Health Behaviors and Behavioral Economics in the Context of HIV, Malaria, and Exercise

HEALTH BEHAVIORS AND BEHAVIORAL ECONOMICS IN THE CONTEXT OF HIV, MALARIA, AND EXERCISE

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Abstract

Although the challenges of population health differ widely between rich and poor countries, fundamental features of health behavior shed light on how individuals make choices about their health. These insights that can cut across countries and cultures. In this thesis, I apply concepts from behavioral economics to provide insights into how cognitive biases and social influences guide health behavior.

Paper 1 addresses inter-household spillovers and knowledge of HIV status. Using regression discontinuity design and a population-based dataset from South Africa, I estimate how a person's ART eligibility affects their household member's HIV status knowledge. ART led to a large increase in HIV status knowledge among the patient's male household members. Although prior studies have noted a correlation between ART expansion and testing rates, this study is among the first to causally link ART initiation to increased awareness of HIV status among household members.

Paper 2 assesses the role of present bias and salience in malaria prevention behavior and risk perception in northern Ghana. Using lab-in-the-field measurement and high-frequency surveys of market vendors in Tamale, Ghana, I find that time preferences do not predict spending on malaria prevention or bednet utilization, but recent illnesses are associated

with malaria prevention spending. I investigate the role of beliefs about malaria risk and find that respondents whose children had been ill in the past two weeks report higher subjective expectations of malaria risk, suggesting that recent episodes of illness may increase an individual's perception of risk and lead to increase spending on malaria prevention.

Paper 3 uses a behavioral field experiment to evaluate whether personal, goal-oriented reminders are an effective means to increase exercise frequency. I ran a 12-month randomized controlled trial on members of a chain of gyms in Montreal, Quebec. The trial compared generic SMS reminders with personalized reminders that recalled members' own exercise goals, which were elicited via a questionnaire at the time of study enrollment. I find that individuals who received personalized reminders did not exercise more frequently than the general reminder group and present suggestive evidence that recalling their goals generated a discouragement effect.

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1

Introduction

Although the challenges of population health differ widely between rich and poor countries, fundamental features of health behavior that shed light on how individuals make choices about their health can cut across diseases and cultures to inform global health policy. Behavioral economics can provide global health policy makers, practitioners, and researchers a useful lens through which to analyze health behaviors. As economists began to borrow theories from psychology in order to improve their models of human behavior and allow for some important deviations from the standard model of economic behavior, they have opened up new ways of modeling choice and behavior which are relevant for understanding the complex decision making around health. Public health challenges that involve complex, habitual behaviors, such as handwashing, healthy eating, exercise, and medication adherence require sustained effort on the part of individuals, in contrast to areas where changes to the environment (municipal water treatment) or one-time investments (vaccines) are available. Therefore, understanding health behaviors, and the preferences and biases that underlie them, is critical to informing successful public health programs and policies.

This dissertation focuses on three important challenges facing global health: (1) Expanding take-up and adherence to HIV medication worldwide as HIV care transitions to treating chronically ill patients who live longer, healthier lives. (2) Increasing malaria prevention to continue the significant progress made in the past decade. (3) Increasing rates of physical activity in developed countries to reduce the burden of chronic disease.

Since effective treatment for HIV was developed, the global agenda has focused on expanding access to care around the globe, with a focus on the hardest-hit countries and populations. Now, global guidelines are shifting away from restricting access to antiretroviral therapy (ART) based on disease state, and moving toward a universal test-and-treat approach that would immediately offer ART to HIV-positive individuals as soon as they learn their HIV status [1]. Because ART is highly effective at reducing HIV-related mortality [2], HIV-positive people are living longer, healthier lives and will continue to need HIV treatment on a continuous basis. In this way, HIV care will begin to resemble treatment for chronic disease. Continued success and progress toward ending the HIV epidemic will rely on engaging all HIV-positive individuals in care. Historically, men have been harder to engage in HIV treatment and the disparity in treatment seeking between men and women are an important factor in explaining higher rates of HIV-related mortality among men, and also contribute to further disease spread [3]. A nuanced understanding of health behaviors, with a particular focus on populations that have been historically difficult to engage in testing and treatment, is a crucial first step toward designing effective programs and policies.

Malaria is an important contributor to morbidity and mortality in sub-Saharan Africa. In 2016, malaria was responsible for 10% of disability adjusted life years (DALYs) in sub-Saharan Africa, and 18.5% of deaths among children under 5 [4]. Combating malaria was a cornerstone of the Millennium Development Goals, and massive investments in global health programs (2.9 billion USD in 2015 [5]) have fueled efforts to distribute insecticide-treated nets (ITNs) and subsidize the most effective class of drugs for treating malaria. Enormous progress has been made: since 1990, deaths among children under 5 in sub-Saharan Africa have declined by 50% [4]. However, there is no vaccine for malaria so people living in highly endemic environments are continuously exposed to the disease. Therefore, malaria prevention is an important component of the global fight against malaria. ITNs are highly effective at preventing malaria transmission [6, 7] but, unlike a one-time vaccine or municipal vector control measures, requires habitual use in order to provide sustained protection against infection. Despite high rates of ITN coverage, rates of utilization are uneven [5]. Detailed information on individual preferences, beliefs, and biases and their relationship to malaria prevention behavior can help to explain determinants of malaria prevention behavior and assist in designing interventions to increase prevention.

As health improves around the globe, chronic diseases are an increasingly important contributor to population health in both developing and developed countries [8]. Physical activity is an important behavioral risk factor for a range of chronic conditions [9]. Yet only 1 in 5 Americans meets the federal guidelines for adequate physical activity [9]. Further, health habits like exercise require ongoing effort and do not yield immediate results, making them particularly difficult to sustain. Interventions that successfully change exercise habits are difficult to design, with most programs only producing modest results [10]. However, even individuals who join gyms, indicating that they intend to exercise, are unable to exercise as often as they predict [11]. Designing interventions that target specific features of individual preferences and biases simultaneously helps to illuminate complex health behaviors and provide evidence for the types of interventions that may be successful in addressing this challenge.

All three of these global health challenges require a deeper understanding of complex health behaviors. Research into the preferences, beliefs, and biases that inform behavior cut across countries and cultures. In this thesis, I apply concepts from behavioral economics to provide insights into how preferences, cognitive biases, and social influences guide health behavior. My first paper addresses intra-household spillovers and knowledge of HIV status. My second paper assesses the role of present bias and salience in malaria prevention behavior and risk perception. My third paper uses a behavioral field experiment to evaluate whether personal, goal-oriented reminders are an effective means to increase exercise frequency.

Overview of this dissertation

Paper 1: Does HIV treatment availability encourage people to learn their HIV status?

A crucial first step toward obtaining HIV care is knowing one's own HIV status. A large proportion of HIV-infected people in South Africa do not know their status, and men are typically less likely than women to be aware of their serostatus. Expansion of HIV treatment may increase disclosure, reduce stigma, and increase testing.

In this chapter, I estimate how a person's eligibility for ART (antiretroviral therapy) affects their household member's HIV status knowledge. I use a regression discontinuity design (RDD) that exploits the CD4 count threshold for ART eligibility in South Africa to evaluate its effect on the patient's household members self-reported HIV status knowledge. ART led to an increase in HIV status knowledge among the patient's male household members. The effect was concentrated among men living in households where women became eligible for ART, and there was no effect for female household members. Although prior studies have noted a correlation between ART expansion and testing rates, this study is among the first to causally link ART initiation to increased awareness of HIV status among household members. This effect may be due to increased testing, or to updating of beliefs about HIV status based on partner's status even in the absence of test results. In designing the next generation of ART programs, such household-level spillover effects could be harnessed to increase HIV status knowledge and ART uptake among men.

Paper 2: Present bias, salience, and malaria prevention in Ghana

Individuals often do not engage in enough preventive health behaviors, even when preventive services are low-cost and spending on curative care is relatively high. Cheap preventive technologies are available for many illnesses that are important contributors to the global burden tof disease, such as malaria, diarrheal disease, and vaccine-preventable illnesses. Public health programs and policies have traditionally aimed to improve access and reduce financial barriers, to counter constraints such as lack of information or financial resources. Behavioral economics offers another lens through which to view low engagement in preventive behaviors, which may contribute to a more nuanced understanding of the problem. Behavioral economics borrows theories of behavior from psychology and models the cognitive and behavioral biases that prevent individuals from engaging in optimal behaviors.

In this paper, I focus on two behavioral explanations of decision-making, time preference and inattention. I evaluate the relationships between time preferences and salience on two measures of malaria prevention: spending on malaria prevention products and ITN utilization. Using data from a baseline survey and nine rounds of follow-up surveys, I use linear regression and individual fixed effect models to evaluate the association between time preferences, salience, and increases in malaria prevention. To explore the mechanisms that underpin the role of salience in malaria prevention, I assess the association between salient illness episodes and beliefs about malaria risk. I find that time preferences do not predict spending on malaria prevention or bednet utilization, but recent salient illnesses substantially increase spending on malaria prevention. I find that respondents whose children had been ill in the past two weeks report higher subjective expectations of malaria risk, suggesting that recent episodes of illness may increase an individual's perception of risk and lead to increase spending on malaria prevention. These findings highlight the importance of understanding the role of behavioral biases in malaria prevention, and may contribute to intervention design to increase preventive behaviors in highly endemic environments.

Paper 3: Making it personal: The effect of personal goals in SMS reminders for gym attendance

Individuals often fail to exercise regularly, and low levels of physical activity are an important contributor to the chronic disease burden in developed countries and, increasingly, around the world. Even individuals who desire to exercise have difficulty doing so. There are many potential explanations for why individuals do not exercise as frequently as they want to. Time inconsistency, a behavioral bias wherein people weigh immediate costs highly and discount future benefits, is a potential explanation for the "intention-action gap" observed in gym attendance. Similarly, people may fail to exercise because they have many competing priorities and exercise is not the most salient issue. Low-cost interventions such as reminders might help individuals to make better decisions. Reminders have been applied to a wide range of health behaviors, but more information is needed on how to design the content of the reminders.

This study explores one potential way of enhancing reminders' effectiveness by leveraging behavioral biases. I compare the effect of two types of SMS reminders that encourage gym attendance: A personal goal reminder that explicitly benchmarks participants against a previously-stated goal in order to motivate them to exercise, and a simple, general reminder to exercise that contains no personalized information. I ran a 12-month randomized controlled trial on members at 12 locations of a chain of gyms in Montreal, Quebec. The trial compared the two SMS reminders effects on goal attainment and weekly gym attendance. I find that the personal goal reminder was not more effective than the generic reminder, and that neither reminder had a measurable effect in the sample overall. However, the effect of the personal goal reminder was significant for individuals whose goals were set at an attainable level, compared to those whose goals were set much higher than their average pre-intervention attendance, suggesting that recalling their goals generated a discouragement effect. This study adds to the growing literature on behavioral interventions aimed at healthy habits.

2

Does ART increase HIV status knowledge among family members? Evidence from a population cohort in rural South Africa

Introduction

The increased availability of HIV antiretroviral therapy (ART) in the past decade has changed what it means to be diagnosed with HIV, even in the poorest countries. ART not only prolongs life but also reduces disease transmission [12]. More than 18 million people are now receiving ART as of 2016, representing approximately half of all people living with HIV [13]. However, there remains a large HIV-infected population not on ART and at risk for onward transmission.

The international organizations concerned with HIV, UNAIDS and WHO, have recommended universal test and treat policies in which all people are encouraged to test frequently and to start ART at diagnosis regardless of the patient's disease state or CD4 count [1]. Many countries world-wide, including in sub-Saharan Africa, have recently adopted universal test and treat policies. To guide progress, UNAIDS developed the 90-90-90 targets which aim for 90% of HIV-positive individuals to know their status, 90% of those testing positive to enroll on ART, and 90% of those on ART to be virally suppressed by 2020, in order to end the HIV epidemic by 2030 [14]. This strategy underscores the importance of HIV testing and status knowledge as a cornerstone of the plan to end the epidemic.

The scale-up of ART has coincided with an increase in HIV testing and care-seeking. However, it is unknown whether the rise in HIV testing coinciding with ART scale-up reflects a causal effect of ART uptake or the impact of contemporaneous HIV testing campaigns. One plausible reason for this relationship is that people who take up ART facilitate HIV testing and HIV status knowledge among their family members. Because ART involves life-long daily medication, it may lead to more conversations among family and friends about HIV; it may destigmatize the epidemic; and it may improve attitudes towards the health system in general. "Social exposure" to ART in families [15] may allow people to directly observe that ART is accessible, safe, and effective, and thus provide motivation to learn one's own HIV status. In contrast, it is also possible that ART reduces the motivation to know one's HIV status among family members. Side effects from ART, the hassle of daily medication, and costs of care-seeking [16] may discourage HIV testing as knowledge about ART proliferates. Discouragement from learning that one is HIV positive but not yet eligible for treatment may be a barrier to care seeking, which hopefully will be lessened as global policy moves toward implementing the universal test and treat model. Further, it is possible that negative interactions with the health system of some family members – for example, being chided for imperfect ART adherence – may discourage HIV status knowledge among family members.

In South Africa, there has been progress in improving HIV status knowledge in the population, but testing rates remain low relative to global targets and are particularly low among men [17, 18, 19, 20]. Low rates of testing and entry into care for men are a driving factor for the high lingering burden of HIV mortality among men [3], and may contribute to the continued high incidence of HIV among women. Several potential explanations exist for why testing rates, and other HIV-related cascade-of-care and health outcomes, are worse for men, including concepts of masculinity [21], the early focus in the HIV response on PMTCT and ART linked to antenatal care [22], and gender-specific stigma [23].

One potential avenue for engaging people, and especially men, in the HIV care cascade is to reach them via their family members who have tested positive and begun HIV treatment. When an individual member initiates ART, several pathways might lead their family members to be more likely to know their own HIV status: they may revise their beliefs about their own risk of infection or about the benefits of receiving treatment and thus decide to get tested; they may be encouraged to test by new knowledge of how to access testing and treatment; or they may simply assume that they are also positive without actually testing.

In this paper, we aim to answer the question of whether ART encourages – or discourages – family members of the ART patient, in particular men, to report knowing their own HIV status. The particular pathway from increasing ART linkage and uptake we are examining is the spillover effect from people enrolled in ART programs to their family members. Positive spillover effects could contribute to moving countries closer to achieving the first of the three 90s among UNAIDS' 90-90-90 targets; negative spillover effects could be obstacles to 90-90-90 achievement.

To identify the causal effect of ART on family members' self-reported knowledge of their own HIV status, we exploit quasi-random variation in ART eligibility based on the individual's CD4 count at clinical presentation using a regression discontinuity design. The quasi-experimental design allows us to control all confounding of the relationship between ART and family members' HIV status knowledge.

Data and Methods

Study context and sample

The data are from routine surveillance carried out by the Africa Health Research Institute (AHRI), a Wellcome Trust-funded research institute in KwaZulu-Natal, South Africa. AHRI's population surveillance system monitors demographic data related to births, deaths, and migration in addition to collecting detailed health, social, economic and behavioral data via population-based household surveys of individuals.

Data collection began in 2003 and the cohort now consists of more than 85,000 people in 11,000 households living in rural KwaZulu-Natal [24]. Both indivduals and households are followed longitudinally. Individual surveys are administered annually to men and women 15 years and older who are residents of the study area. This study uses data primarily from a module focused on HIV knowledge and beliefs, which is administered in the annual individual surveys. Overall participation rates in the household surveys is above 99% [24] but refusal to answer specific modules or questions increases missingness rates of some variables. In addition, AHRI oversees the collection of clinic-based data for HIV patients who seek care in any of the South Africa Department of Health clinics in the study area. This clinic-based data includes information on patient HIV status, CD4 counts, ART enrollment dates, and clinic visit dates, and is individually linked to the individual and household survey data [25].

The study population included all family members of individuals seeking HIV care in the public sector HIV treatment and care program between January 2007 and August 2011. To account for migration and movement between households, individuals were assigned to the family that they lived with at the time that the first member of that family received their first CD4 count, and were followed through 2013. The individual with the first CD4 count in each family (defined in this analysis as the index family member) was excluded from the analysis. Rather, these individuals determined exposure status for the other family members, among whom outcomes were measured. Families and their members were included in the sample if the index family member CD4 count was measured between Jan 2007 and Aug 2011. After 2011 the South Africa national guidelines began to raise the CD4 count threshold for ART eligibility and eventually moved to a universal test and treat policy, so our study uses only data from the period before that policy change. Our sample contains 4,630 families and 25,528 individuals (approximately 30% of the total individuals in the ACDIS cohort). In our sample, 31% of individuals have only one survey round after the index member's first CD4 count, 24% have two rounds, 17% have three, and 28% have four or more.

Exposure and outcome measures

Our primary outcome measures an important early stage of the HIV care cascade: an individual's self-reported knowledge of their HIV status. Administrative data on all HIV testing is not recorded in this dataset because clinical outcomes come from HIV care clinics that treat patients who are HIV positive, rather than the general population who may be positive or negative. We are thus unable to assess incident HIV testing and instead use self-reported status knowledge as an outcome. In the household survey, adults ages 15 and over were asked "Do you know your HIV status?" Those who responded "yes" were coded as knowing their status, as opposed to those answering "no" or "do not know". Outcomes assessment was restricted to the five-year period following the date of the index family member's first CD4 count. In our dataset, this outcome is available for the full population-based sample, which is advantageous because we are interested in population-level effects, not only effects among those who have contact with clinics or are already under HIV surveillance. Outcomes later in the care cascade, such as timely ART enrollment and viral load suppression, will be the subject of further research.

The exposure is based on the CD4 count of the first person to obtain a CD4 test in each household. During the period of study, patients were eligible for ART if they presented with a CD4 count <200 cells/mm^3 or with Stage IV AIDS-defining illness. Respondents in the study population were considered exposed if they were members of a family where the index family member had sought care with a baseline CD4 count below 200 cells/mm^3 and were therefore eligibile for ART. Family members of index members whose baseline test was above the 200-cell cutoff were defined as unexposed. We considered only the first CD4 count for the index family member, which is the first test in each family, because decisions about when to re-test may be endogenous to the CD4 count observed in previous tests, and therefore individuals testing above and below the cut-off may no longer be comparable with one another at subsequent tests. Some people may choose to test more frequently, for example, or to test multiple times in a short period if their tests are just above the cut-off. We therefore look at the first test of the first family member to get tested, and then measure outcomes for all other members of that family.

All HIV care in the study area is administered through participating clinics, which use CD4 counts to determine eligibility for ART and pre-ART services. The clinic-based data captures the CD4 counts of patients when they present at the clinics for HIV treatment, but does not capture HIV tests which can be obtained at a variety of clinics and testing facilities in and around the study area. This is why we have access to HIV surveillance data such as CD4 counts and dates of ART initiation for all HIV-positive patients, but we do not have population-based measures of HIV testing behavior for individuals who may be positive or negative. We instead rely on self-reported measures in the population-based household surveys. For further discussion on the self-report and clinical variables, see table 2.3.

Empirical methods

We assessed the relationship between one's self-reported knowledge of HIV status and the index family member's ART eligibility using a regression discontinuity design (RDD). This analytical approach can be used when an exposure is determined by a cut-off value of a continuously-measured variable, such as CD4 count. By exploiting the cut-off rule that determines exposure to treatment, RDD allows researchers to make causal claims under much weaker assumptions that in other quasi-experimental methods [26, 27, 28, 29, 30]. For RDD to produce unbiased causal estimates, the key assumption is that the distribution of outcomes around the cut-off would be continuous in absence of the treatment. If this assumption is met, the discontinuity in the outcome, HIV status knowledge, can be attributed to the exposure, ART eligibility.

When the variable that governs treatment assignment is measured with error, this assumption is automatically met unless individuals are able to perfectly manipulate their assignment variable value [31, 32]. CD4 counts are measured with substantial random error and it is unlikely that patients or providers would be able to manipulate their values. To assess the likelihood of manipulation, we visually inspect the distribution of CD4 counts to assess whether there is "bunching" below the cutoff [33] and carry out a formal test for equality of the distributions on each side of the cutoff by estimating RDD models with the baseline covariates as outcomes of interest [34]. In order to perfectly manipulate CD4 count, patients would need to either shop around for multiple CD4 counts until they obtain a measurement below 200, or convince the lab to somehow alter the test result. We avoid the first problem by only using the baseline (first) test result in determining exposure status. The second scenario, while possible, is unlikely in a context where lab results are directly entered into the data management system as they are processed.

As a result of the assumption of continuous outcomes in absence of the treatment, individuals on either side of a small window around the cut-off are expected to be similar in all observable and unobservable characteristics. We perform balance tests on either side of the cutoff to assess exchangeability based on observable characteristics. This test consists of estimating a discontinuity at the cutoff for observable characteristics [35] (age and sex) to evaluate whether the covariates differ on either side of the cutoff.

Many individuals do not enroll in ART even when eligible, and other eligibility guidelines grant access to ART even when CD4>200 (for example, in pregnant women or cases of TB co-morbidity). We therefore use a "fuzzy" RDD estimator, which is analogous to scaling the intent-to-treat effect by the compliance rate in a randomized trial with imperfect compliance [27]. See appendix A for further details.

We first estimate the intent-to-treat effect in a regression of the following form:

$$Y_{it} = \beta_0 + \beta_1 1[Z_{it} - c] + \beta_2(Z_{it} - c) + \beta_3 1[Z_{it} - c](Z_{it} - c) + \varepsilon_{it} \tag{2.1}$$

where Y_{it} is the outcome of interest, β_1 is the difference in the outcome at the cutoff (the effect of ART eligibility), β_2 is the slope of the line below the cutoff, and $\beta_2 + \beta_3$ is the slope of the line above the cut-off. Z_{it} is the index family member's baseline CD4 count and c is the cut-off value (200), so the indicator $1[Z_{it} < c]$ is equal to 1 when the CD4 count is below 200 and zero otherwise. The intent-to-treat estimate can be interpreted as a local average treatment effect (LATE), meaning that it is the average effect among all observations near the cutoff point, regardless of whether they complied with the treatment.

We then use the fuzzy RDD estimator, where we scale the effect of eligibility by the compliance rate to obtain the effect of ART using the following model:

$$Y_{it} = \beta_0 + \beta_1 ART_{it} + \beta_2(Z_{it} - c) + \beta_3 1[Z_{it} - c](Z_{it} - c) + \varepsilon_{it} \tag{2.2}$$

where instead of using an indicator variable for CD4<200, as in the previous model, we use a variable for ART enrollment that has been instrumented using treatment assignment based on CD4 count. This gives us the complier average causal effect (CACE), which can be interpreted as the average effect among those who complied with their treatment assignment (i.e. took up ART because they were below 200, or did not because they were above.)

Standard errors are adjusted for clustering at the household level. We disaggregate results by the gender of the family member to evaluate the effect on men.

We also carried out sensitivity analyses and falsification tests. Because of the local nature of RDD estimation, the aim is to use data as close as possible to the cutoff to ensure exchangeability. The optimal bandwidth is chosen using a data-driven selection procedure designed to optimize the trade-off between bias and variance [36, 37], and sensitivity to bandwidth choice is assessed using a range of bandwidths, and reported in the appendix. We further carried out a placebo test where false cutoff values were utilized instead of the true value to demonstrate that the discontinuity occurs only at the true cutoff value. Inclusion of covariates in RDD models is generally not necessary with RDD models because small bandwidths restrict the data to near the cutoff, where it is assumed that observations are exchangeable. Calonico et. al. [35] note that while it is possible to include covariates, it is often avoided due to the risk of the RDD becoming inconsistent if the functional form is misspecified. For this reason, we do not use covariates as a sensitivity analysis.

Table 2.1: Pre-exposure sample characteristics

	Index members % or Mean(SD)	Other members % or Mean(SD)
Age	32.15(12.20)	28.57(18.00)
Female	77.41	56.31
Knows HIV status	5.70	17.72
Has ever tested for HIV	6.80	22.82
Number of observations	4630	20898

Each observation appears in the table once, at the timepoint closest to the index member's CD4 test. Age is calculated and reported at the time of the first CD4 test in the household. 'Knows HIV status' and 'Has ever tested for HIV' are reported at the latest available survey round prior to the first CD4 test in the household. Index members are the first to obtain CD4 tests in each household. Other members are the family members living in the same household at the time of the first CD4 test.

Results

Sample characteristics

The sample consists of 4,630 household and 25,528 individuals. Table 2.1 shows baseline sample characteristics for the index member and other household members prior to exposure, i.e. prior to the household's first CD4 test. Average age at baseline is slightly higher among the first testers in each household (index family members), at 32.15 years, relative to 28.57 years among their family members. 77% of index members are female, whereas their family members are about half female (56%). Baseline (pre-exposure) rates of HIV status knowledge and self-reported testing are lower among the index members relative to their family members. 5.7% of index members, and 17.72% of family members, report knowing their HIV status. Similarly, 6.8% of index members and 22.82% of family members report that they have ever been tested for HIV. These discrepancies may be due to true differences in HIV knowledge and testing behavior: these rates are from the period before the index tester obtains the household's first CD4 count, so the index member may not yet know their HIV status due to not yet having tested. The discrepancy may also be the result of differential tendencies to accurately report in the survey (perhaps due to social desirability bias) or other characteristics like differences in sexual behavior and HIV risk. The overall low rates of self-reported testing and status knowledge may be due to the fact that these are measured prior to exposure, and thus are by definition before the index member enters into care and are also primarily from the earlier years covered by the sample when ART scale up was not yet as widespread.

Prior to the first CD4 count in each family, male and female family members report very different levels of knowledge of their own status and of prior testing behavior, shown in table 2.2. Women are twice as likely as men to report that they know their own HIV status (22% vs 11%, p<0.01). They are also 8 percentage points more likely to report that they have ever been tested for HIV, a relative increase of 44% over male respondents (p<0.01).

Table 2.2: HIV testing and status knowledge among HH members, prior to exposure

	Men		Women		P-value of test of
	Mean	SD	Mean	SD	difference in means
Knows HIV status	0.11	0.31	0.22	0.42	<0.01
Has ever tested for HIV	0.18	0.39	0.26	0.44	<0.01

Both variables reported for household members, not index member, at the latest survey round prior to the first CD4 test.

Table 2.3 shows the rates of self-report of prior HIV tests and HIV status knowledge among index members, before and after their first CD4 test. Because we have clinic data for all the index members, we know the date of their initial CD4 test and therefore the date at which the individual themselves both knows their status and has had at least one HIV test. This allows us to compare responses in the household survey of a group that should self-report prior HIV tests and that they know their own HIV status. Table 2.3 shows that, prior to the first CD4 test, the overwhelming majority of this group reports that they have neither tested for HIV nor know their status (92%). After the CD4 test, there is a large increase in the proportion reporting that they know their status, increasing from 5.71% to 51.15%. There is a smaller increase among those reporting that they have ever been tested for HIV, from 6.84% to 18.07%. We also observe that 35% of index testers report that they do know their status but have *not* tested for HIV, when clinic data confirms that these individuals are HIV positive. Taken together, these figures indicate that the self-reports are correlated with behavior that we observe in the clinics (CD4 tests) but that the question about ever testing for HIV is not as sensitive as the question about knowing one's HIV status.

Table 2.3: Self report vs. clinic-based data among index HH members

		Knows HIV status		
	Has tested for HIV	No	Yes	Total
	No	92.38	0.77	93.15
Before CD4 test	Yes	1.90	4.94	6.84
	Total	94.28	5.71	100
	Has tested for HIV	No	Yes	Total
	No	46.54	35.38	81.92
After CD4 test	Yes	2.30	15.77	18.07
	Total	48.84	51.15	100

Responses are reported for all index household members, at the last survey round prior to their CD4 test (Before CD4 test) and at the first survey round post-test (After CD4 test).

Validity of assumptions

Regression discontinuity designs require that a few assumptions be met. We begin by assessing whether manipulation of CD4 count is likely, which would indicate a violation of the continuity of outcomes in absence of the treatment. Figure 2.1 displays the distribution of first CD4 counts for every family. We do see a slightly higher density of CD4 counts below 200. A formal test, following Cattaneo [34], failed to reject that the distributions of CD4

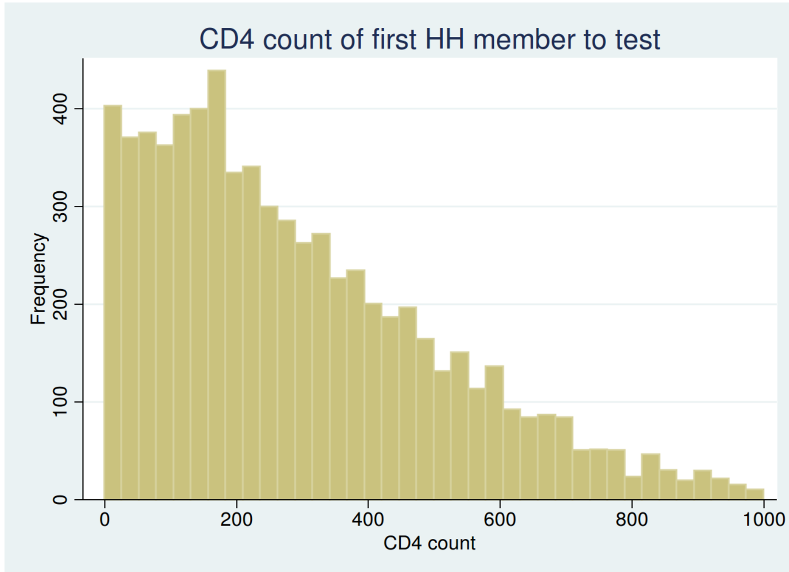

Figure 2.1: Distribution of baseline CD4 count for index family member

counts are equal on either side of the cut-off (p=0.75).

Tests for differences in observable characteristics (age and sex) around the cut-off reveal no significant difference for sex (p=0.562) and a small, but statistically significant, difference in age (1.2 years, p=0.058). We therefore include age as a covariate in our sensitivity analyses.

Main outcomes

In table 2.4 we report the effect of ART on family members' knowledge of their HIV status. Panel A reports the LATE, analogous to intention-to-treat effects. We find that there is no impact in the full sample, but that when disaggregated by sex of the family member, men increase their self-reported knowledge of HIV status when living with someone who is eligible for ART. Men who live in a family where the baseline CD4 count was below 200 are 9.1 percentage points more likely to report that they know their HIV status. This is a

Table 2.4: RDD estimates of ART on HIV status knowledge

	Sample		
	All	Women	Men
Panel A: Local average treatment effects			
Effect estimate	0.05	0.00	0.11***
Std Error	0.03	0.04	0.03
P-value	0.104	0.906	<0.001
Num observations	17102	9870	6046
Num clusters	2801	2252	1987
Bandwidth	63	61	55
Panel B: Complier average causal effects			
Effect estimate	0.19	0.01	0.47**
Std Error	0.14	0.28	0.21
P-value	0.185	0.896	0.029
Num observations	24078	9678	6859
Num clusters	3600	2325	2131
Bandwidth	90	60	63

All models estimated using local linear regression. Standard errors clustered at the household level. Effect sizes can be interpreted as percentage points. *** $p<0.001$, ** $p<0.05$, * $p<0.1$.

large relative increase in relation to the pre-treatment average of 11%. Panel B reports the treatment-on-the-treated or Wald estimates, which are obtained by scaling the ITT effects by the compliance rate to estimate the treatment-on-the-treated effect. As with the results in Panel A, the effect is not significant for the full sample or for women. But the effect for men is large and significant, representing an increase of 45 percentage points for men who live with a family member who is enrolled on ART. The treatment effect can be visualized by plotting the outcome on the index member's CD4 count, shown in figure 2.

We carried out a placebo test by running the same model using false cutoff points (CD4 counts of 150, 250, and 300), reported in table 3. No significant effects of the placebo cut-offs (at CD4= 150, 250, and 300) were found, supporting the robustness of our RDD. Finally, we examined alternative outcomes in the domains of HIV knowledge and care-seeking behavior to determine if the changes in status knowledge may correlate with other health outcomes. There was no significant association with any of the alternative outcomes. For further details see appendix B.

Discussion

An ambitious goal of ending the HIV epidemic by 2030 requires that nearly all HIV-positive individuals learn their status. HIV status knowledge in many of the HIV hyperendemic communities in sub-Saharan Africa still falls far short of this target, in particular among men. One avenue to encourage men to learn their HIV status is through their family members. In this study, we used a regression discontinuity design to estimate the effect of ART on the self-reported knowledge of HIV status among a patient's family members. We found that living with an individual who is eligible for ART resulted in an 11 percentage point increase in the probability that male family members report knowing their HIV status.

Men may be encouraged to test and learn their status by seeing that a family member is eligible for treatment, or they may report knowing their status without testing by inferring their status based on their partner's status. In order to learn one's HIV status, several steps are required. First, an individual must decide to test and then obtain test results. If the test is positive, the person must trust the results and update their beliefs accordingly. If the test is negative, the person must re-test frequently to maintain certainty of their negative status. We do not have access to clinic-based data on all HIV tests in the study population so we cannot corroborate the self-reports with clinic-based data or measure incident HIV testing rates and therefore cannot disentangle whether the increased HIV status knowledge we observe is the result of increased testing or inferred status based on a partner's diagnosis.

If the self-reports reflect true increases in HIV testing rates, this may be due to increased familiarity with the health system via the family member or the direct encouragement from health providers for patients to encourage their partners to test. It may also be due to reduced stigma surrounding treatment seeking for men in particular, or an increased sense of urgency around HIV testing if the individual's sexual partner began ART.

Men enter into HIV care at more advanced disease states and are more likely to be lost to follow up [38], and experience higher rates of mortality due to HIV [39]. This suggests that men face particular issues in seeking HIV care, which is why the results of this study are relevant for policy. A qualitative study of HIV patients and health workers in Zimbabwe found that conceptions of masculinity may discourage men from seeking care if they are in conflict with the steps required to obtain care, such as becoming a patient who follows instructions from (mostly female) nurses and practices safe sex [40]. In Uganda, Bwambale and colleagues [21] analysed focused group discussions where men cited concepts of male superiority as reasons for not testing for HIV, and found that men assumed they were HIV-positive and therefore did not need to test if their female partners were diagnosed with HIV. Men face specific health system barriers when seeking care for HIV: due to the early emphasis on maternal and child health as a priority area for HIV care, HIV care has traditionally been linked to pregnancy and this image may act as a barrier to men seeking care [22], despite the expansion of ART to the general population.

The success of universal test and treat policies begins with HIV status knowledge and HIV testing. HIV positive individuals in South Africa are most likely to be lost to follow-up between HIV testing and linkage to care [20], which underscores the critical importance of the moment that one learns their HIV status in the HIV care cascade. New innovations in HIV testing seek to address barriers that have discouraged testing in traditional clinic settings, either because of time and transport costs, or perceptions about clinics that may discourage some individuals from seeking care. HIV self-testing and home-based testing with the help of health workers are both strategies that promise to increase testing rates, but whose success may be impacted by family and household dynamics. Understanding these dynamics can inform how these programs are rolled out to target them in the most effective way.

Programs that encourage partner testing have been effective at increasing testing rates among men specifically [41, 42]. Similarly, HIV self-testing dramatically increased the proportion of men who test [43] and is promising for its potential to reach those who will not seek care in a clinical setting. These programs and technologies are new, however, and little is known to date about how to design programs so that they reach asmany people as possible. Leveraging family relationships may help to reach men, either by encouraging them to test in traditinal settings or potentially through distribution of self-test and through partner testing. These programs may engage more people in HIV care by casting a wider net and leveraging many types of household and family relationships to, for example, distribute self-testing kits, as opposed to focusing only on sexual partners.

Our study has strengths and limitations. An important strength is the use of a regression discontinuity design, which allows for causal effect estimates even when using observational data, which is especially important when evaluating spillovers at the family level where unobservable characteristics of family members' health behaviors are likely to be correlated. The study is limited by our use of a self-reported outcome. We are unable to directly measure an individual's HIV testing behavior so we rely on self-reported knowledge of HIV status. This could be subject to social desirability bias or otherwise inaccurately reported due to stigma surrounding HIV. We also cannot differentiate between people who know their status with certainty (are HIV positive) from those who may report knowing their status despite not having tested recently and who therefore may incorrectly believe that they know their status. Knowing one's HIV status is the first step toward obtaining care, but individuals must act on this information and seek treatment. In South Africa, the early stages of the HIV care cascade experience the highest loss to follow up [20], indicating that improvements in linkage to care are needed in order for increased testing and status knowledge to produce better health outcomes. Finally, we take advantage of the ART eligibility guidelines for our identification strategy, but as global HIV treatment policy has shifted the changing guidelines limit the generalizability of our results. As countries implement universal test and treat policies, individuals may become more eager to test because of the immediate availability of ART. This may affect how family and household relationships influence testing decisions.

Conclusion

This study evaluated the effect of ART on knowledge of HIV status among a patient's family members using regression discontinuity design. Our results show that men are 11 percentage points more likely to report knowing their HIV status when they live with a family member who is eligible for ART. This offers an opportunity to leverage family and household relationships to bring more people into HIV testing and care, which is a crucial step toward attaining the levels of treatment necessary to envision an end to the HIV epidemic.

Appendix A: Validity of RDD assumptions

We evaluate the validity of key RDD assumptions, as discussed in section 2.2.3. First we assess the distribution of CD4, the assignment variable, to evaluate whether manipulation is likely. The histogram of CD4 counts is shown in figure 2.1 in section 2.3.2. We also performed a formal test of bunching, which uses a local polynomial to estimate the density on each side of the cutoff and then tests whether there is a discontinuity [34]. The test showed no evidence of a discontinuity in the index member's baseline CD4 count at the CD4=200 cutoff, with a T-statistic of -0.85, and p-value of 0.392.

We also carry out a formal test for balance of covariates across the cutoff. This test estimates RDD coefficients at the cutoff for baseline covariates, at various bandwidths, to test whether they also display a discontinuity. In table 2.5 we see that there is no significant discontinuity in either sex or age at the CD4=200 cutoff. This supports the argument that covariates are balanced on either side of the cutoff and therefore that the groups on either side of the cutoff are exchangeable.

Figure 2.2 shows the probability of initiating ART within two years based on the first CD4 count, for all index household members. This graph demonstrates that there is a discontinuity at the CD4=200 cutoff, and that there is non-compliance on both sides of the cutoff, requiring a fuzzy RDD.

Table 2.5: Covariate balance across the cutoff

Covariate: Female			
Bandwidth	RDD estimate	Robust SE	P-value
25	-0.57	1.30	0.657
50	0.00	4.70	1.000
100	0.29	1.06	0.779
Covariate: Age			
Bandwidth	RDD estimate	Robust SE	P-value
25	-29.33	51.82	0.571
50	-71.63	86.86	0.410
100	-7.91	24.04	0.742

Both covariates are measured among other household members at the last survey round prior to the first CD4 test. RDD estimates reported are obtained using the same RDD estimation procedure as in the main results, shown in equation 2.1. Standard errors are clustered at the household level.

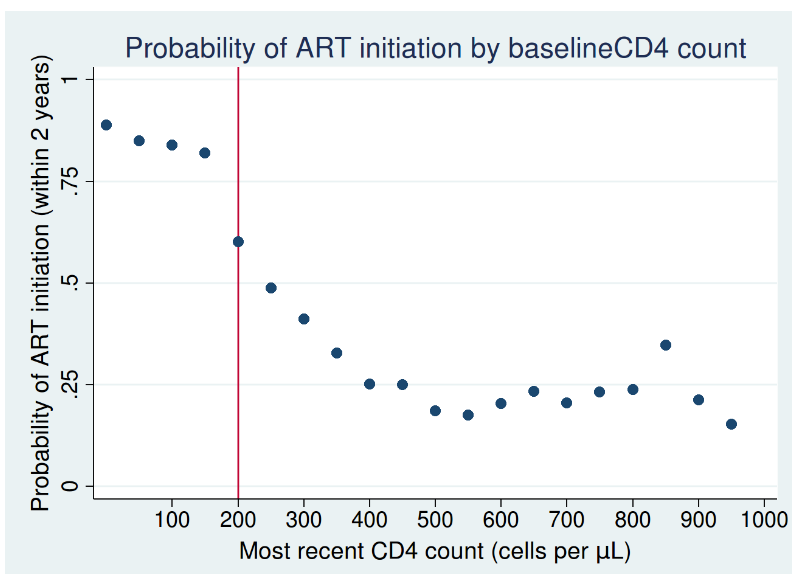

Figure 2.2: ART initiation by baseline CD4 count

Appendix B: Results

Table 2.6: RDD intention-to-treat estimates of ART on HIV status knowledge

Sample: All

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
63	0.05	0.03	0.104	-0.01	0.1	10243	6859	1618	1183
16	0.14	0.06	0.025	0.02	0.27	2132	1797	255	207
31	0.09	0.05	0.062	0	0.18	4825	3483	524	413
94	0.05	0.03	0.087	-0.01	0.11	15180	9769	1540	1131

Sample: Female household members

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
61	0	0.04	0.906	-0.07	0.08	5942	3928	1307	945
16	0.09	0.09	0.32	-0.09	0.26	1243	1078	208	177
31	0	0.06	0.982	-0.13	0.12	2847	2048	438	354
94	0.01	0.04	0.889	-0.07	0.08	9001	5744	1320	960

Sample: Male household members

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
55	0.11	0.03	0.001	-0.17	-0.04	3635	2411	1172	815
16	0.2	0.06	0.002	0.07	0.32	889	719	180	149
31	0.19	0.05	0	0.1	0.28	1978	1435	375	288
94	0.11	0.03	0.001	0.05	0.18	6179	4025	1148	800

All models estimated using local linear regression. Standard errors clustered at the household level. Effect sizes can be interpreted as percentage points. The first bandwidth for each sub-sample is the optimal bandwidth, and then 25%, 50%, and 150% of the optimal bandwidth for the full sample, 90, were chosen for sensitivity analyses. Number of observations (N) and number of clusters given above and below the cutoff.

Table 2.7: Fuzzy RDD estimates of ART on HIV status knowledge

Sample: All

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
90	0.19	0.14	0.185	-0.11	0.54	14716	9362	2069	1531
22.5	0.98	0.59	0.096	-0.17	2.13	3334	2627	371	314
45	0.59	0.39	0.129	-0.17	1.34	7294	5052	773	589
135	0.21	0.16	0.191	-0.11	0.53	20487	13151	2045	1514

Sample: Female household members

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
60	0.01	0.28	0.896	-0.6	0.68	5853	3825	1345	980
22.5	0.38	0.89	0.666	-1.36	2.13	1954	1542	309	271
45	0.13	0.52	0.803	-0.89	1.15	4381	3029	658	506
135	-0.04	0.22	0.855	-0.48	0.4	12121	7756	1778	1286

Sample: Male household members

Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
63	0.47	0.21	0.029	0.05	0.88	4084	2775	1253	878
22.5	0.86	0.53	0.103	-0.17	1.9	1380	1085	273	222
45	0.8	0.43	0.063	-0.04	1.64	2913	2023	554	410
135	0.47	0.2	0.021	0.07	0.87	8366	5395	1517	1078

All models estimated using local linear regression. Standard errors clustered at the household level. Effect sizes can be interpreted as percentage points. The first bandwidth for each sub-sample is the optimal bandwidth, and then 25%, 50%, and 150% of the optimal bandwidth for the full sample, 90, were chosen for sensitivity analyses. Number of observations (N) and number of clusters given above and below the cutoff.

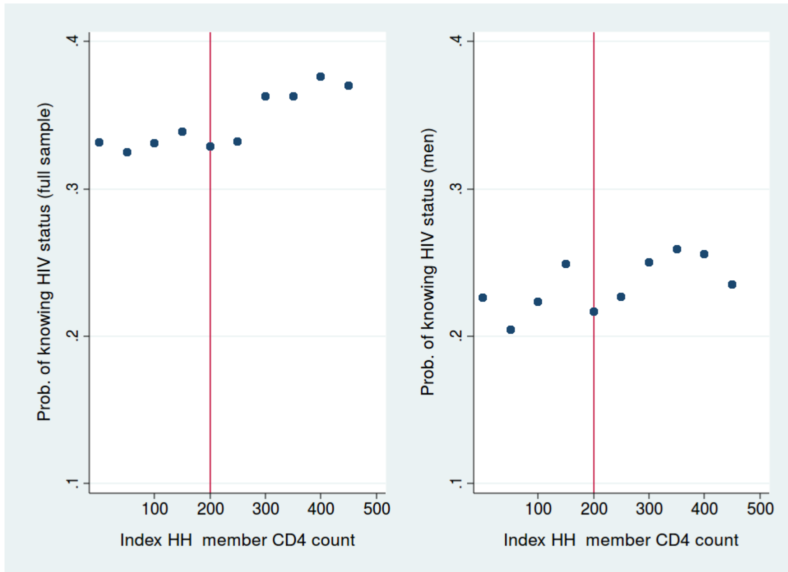

Figure 2.3: Probability of knowing own HIV status, for full sample (left) and for men (right)

Table 2.8: RDD estimates for placebo test using false cutoffs

Sample: Male household members										
False CD4 cutoff	Band-width	Effect	Std. Err.	P-val	95% CI		N below	N above	Clusters below	Clusters above
150	64	0.33	1.04	0.742	-2.02	2.84	3933	3934	1038	1045
250	63	-1.62	2.81	0.566	-8.4	4.59	2909	2475	1157	773
300	64	-5.8	33.85	0.993	-77.39	76.73	2591	2032	928	699

All models estimated using local linear regression. Standard errors clustered at the household level. Effect sizes can be interpreted as percentage points. False cutoffs at CD4=150, 250, and 300 as opposed to the true cutoff of 200. Number of observations (N) and number of clusters given above and below the cutoff.

Table 2.9: RDD intention-to-treat estimates of ART on HIV beliefs and care-seeking

Sample: All

Variable	Effect size	Std. Err	P-val	Bandwidth
Has heard of ART	0.025	0.112	0.33	61
Knows where to get ART	0.020	0.026	0.44	68
Believes ART improves health	0.14***	0.027	0.00	53
Sought care in clinic	0.05	0.03	0.12	41

Sample: Female household members

Variable	Effect size	Std. Err	P-val	Bandwidth
Has heard of ART	0.01	0.03	0.83	59
Knows where to get ART	0.00	0.03	0.99	61
Believes ART improves health	0.01	0.036	0.69	56
Sought care in clinic	0.04	0.04	0.33	40

Sample: Male household members

Variable	Effect size	Std. Err	P-val	Bandwidth
Has heard of ART	0.05	0.03	0.11	66
Knows where to get ART	0.05	0.04	0.17	63
Believes ART improves health	0.02	0.04	0.55	62
Sought care in clinic	0.06	0.04	0.15	35

All models estimated using local linear regression. Standard errors clustered at the household level. Effect sizes can be interpreted as percentage points. Sought care in clinic indicates that the household member ever presented for treatment in an HIV clinic.

3

Present bias, salience, and malaria prevention in Ghana

Introduction

A common feature of health behavior is that individuals do not engage in enough preventive health behaviors even when preventive services are low-cost [44, 45]. In poor countries, spending on curative care is generally high [44] and use of very inexpensive or even free prevention technologies such as vaccines [46], insecticide treated nets (ITNs) [47], or water purification treatments [48] is modest. The burden of disease from illnesses for which we have cheap preventive options is high: In 2015, malaria, diarrhea, and 5 common vaccine-preventable diseases[1] accounted for approximately 18% of DALYs in sub-Saharan Africa [4]. Even so, take-up rates of preventive health technologies are low and highly price sensitive, often dropping to very low levels when prices increase only slightly [49, 50].

The traditional approach to public health has generally been to provide information about the benefits of health behaviors and to promote policies and programs designed to improve access and reduce financial barriers, to counter constraints such as lack of information or financial resources. For example, free distribution of ITNs are designed to eliminate this financial barrier and are a cornerstone of the WHO's Global Malaria Programme [51]. But greater access or better knowledge may not translate directly into increased utilization of preventive technologies. In a study of mothers and their children in Ghana, De La Cruz et al. [52] found that more accurate knowledge about the causes and risks of malaria was not associated with greater bednet use.

Behavioral economics borrows theories of behavior from psychology and offers another lens through which to view health behavior: there are cognitive and behavioral biases that prevent individuals from engaging in optimal behaviors and impede their ability to carry out their

[1] Malaria: 9.42% of total DALYs; Diarrhea: 7%; Measles, whooping cough, tetanus, diptheria, varicella: 1.5%

own preferred actions [53, 54]. This view contrasts with the standard economic framework in which an individual's preferences and the constraints they face inform their choices, but does not consider the type of "mistakes" that behavioral economics allows for. In this paper, I focus on two behavioral explanations of decision-making, time preference and inattention, and examine their associations with malaria prevention.

Time preferences describe how people trade off consumption between the present and the future. Time preferences have been shown to be associated with health behaviors and outcomes in a variety of settings [55, 56, 57]. Present biased preferences are a particular type of time preference where people over-emphasize costs and benefits realized in the present relative to the future. This causes a conflict between what a person wants to do in a future period and what they end up doing. This can lead to procrastination and other patterns of behavior where people fail to implement their own intentions [58, 59]. An over-emphasis on the present makes present bias especially relevant to preventive health behaviors, which almost always imply incurring a cost now in exchange for an uncertain future benefit.

Limited attention is another type of bias that is particularly relevant in contexts where there are many immediate demands on attention, which causes focus on urgent issues to the detriment of others. Limited attention in decision-making may result in low rates of some desirable behaviors, like saving [60] or preventive health behavior simply because individuals do not have unlimited attention to devote to making optimal choices. Like present bias, inattention is especially relevant for preventive behaviors because prevention addresses future health problems so the benefits are not likely to be as salient in the present, leading those with many competing priorities to turn their attention to more immediate concerns. Some evidence suggests that attention and decision making is affected by context of poverty, where people face many pressing demands [61, 62, 46]. Many studies have attempted to leverage SMS reminders as an inexpensive way to focus attention on a health behavior, for example to increase vaccination rates [63] and to improve adherence to treatment for HIV [64] or malaria [65], and encourage exercise (this dissertation). In addition to simply reminding people, health behaviors may be affected by interventions that increase their salience through focusing attention on risk, as Grover et al. [66] did with individuals at risk for HIV. In a different context, but illustrating the same phenomenon, two studies demonstrate spikes in demand for flood insurance immediately after flooding events [67, 68].

In many cases, present bias and inattention (a lack of salience) can give rise to similar observed behavior. For example, a person may not hang an insecticide treated bednet because the time it takes to hang it and the discomfort of sleeping under it loom larger than the future uncertain benefits of malaria prevention, or they may not hang the net because they simply forgot. It is important to distinguish present bias from inattention because — although they may generate similar behaviors — they have different policy implications. Some interventions may affect both, for example incentive payments help to overcome present bias and also increase the salience of a particular behavior. But others, such as text message reminders, only target inattention.

In addition to behavioral biases, choices about preventive behavior are affected by individuals' beliefs about the causes and symptoms of illnesses and the effectiveness and safety of related prevention. With an illness like malaria, whose symptoms that are hard to distinguish from other illnesses, caregivers may have difficulty accurately identifying cases of malaria without a clinical test, in turn making it difficult to see the effectiveness of preventive measures. This may lead them to discontinue using a bednet (for example) because even when they did use it their child fell sick with an illness that they perceive to be malaria. Studies in Uganda show that drug shop use of rapid diagnostic tests (RDTs) for malaria is insufficient to ensure that those who test positive are treated with ACTs and those who test negative are not [69], and that beliefs about malaria risk do not vary with malaria risk factors such as community prevalence or the age of the child [70]. In Ghana, despite public health campaigns targeting malaria in recent years, knowledge of basic information about malaria is not universal. 84% of women in the 2016 Malaria Indicator Survey identified mosquito bite as the cause of malaria, but only 77% reported that bednets could be used to prevent malaria and 54% could identify fever as a symptom of malaria [71]. In such an environment, malaria prevention itself may be viewed as a risky investment because individuals are unsure that the money they spend on bednets will actually prevent cases of malaria.

In this paper, I assess the relationship between time preferences and salience and preventive health behavior in northern Ghana. Specifically, I evaluate the relationships between time preferences (both discount rates and present bias) and salience (a recent salient illness episode) on two measures of malaria prevention: spending on malaria prevention products and ITN utilization. Using data from a baseline survey and nine rounds of follow-up surveys, I use linear regression and individual fixed effect models to evaluate the association between time preferences, salience, and increases in malaria prevention. To explore the mechanisms that underpin the role of salience in malaria prevention, I assess the association between salient illness episodes and beliefs about malaria risk.

Data and Methods

Study setting and sample selection

Data for this study was nested into data collection for a randomized trial evaluating the impact of financial incentives for using mobile money products. The study was conducted in Tamale, a city in northern Ghana. Tamale is a fast-growing city with a population of approximately 350,000 people in the arid region of northern Ghana approximately 600km north of the capital, Accra. Malaria is the fourth leading cause of death in Ghana [4] and has fallen quickly in recent years. Malaria is endemic in Tamale, with high transmission rates throughout the year and some seasonal variation. Detailed data on the epidemiology of malaria in the Tamale urban area is scarce, but one 2006 study (based on a sample of more than 4000 children in Tamale and the surrounding rural areas) found a malaria prevalence of 61.7% in the wet season and 55% in the dry season [72]. Since 2011 when ITN distribution campaigns covered the Northern region, malaria deaths and hospitalizations have fallen in Ghana [73]. However bednet use remains lower than elsewhere in Ghana, and cultural practices such as late-night economic activity and sleeping outside impede bednet use [74].

The sample was drawn from market vendors in an urban marketplace. Larger businesses (wholesalers and those with permanent market stalls) were excluded, so the sample is representative of typical market vendors selling small-scale goods like vegetables and housewares. 1085 individuals were identified in a census and met the eligibility criteria, and of these 636 were enrolled in the mobile money savings study. The analysis sample for this paper consists of the 636 for whom baseline survey and lab data was available.

Data

At baseline, participants completed a short questionnaire that focused on their recent health spending, savings behavior, business characteristics, and basic socio-demographic characteristics. The day following the survey, they completed a lab-in-the-field session where they participated in a variety of survey modules and behavioral games to measure a wide range of characteristics including time and risk preferences, cognitive skills, income variability, and

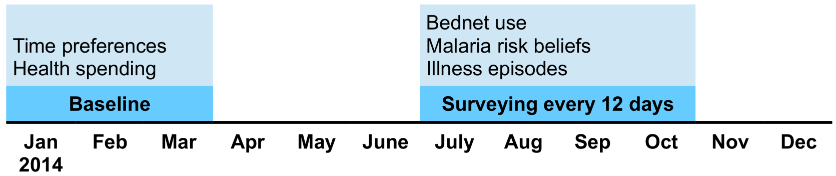

Figure 3.1: Timeline of data collection and measurement of key indicators

projection bias. Subsequently, 9 rounds of a short survey were collected every 12 days. See figure 4.1 for a timeline of data collection activities. All data collection was carried out by staff from the Innovations for Poverty Action Northern Ghana office, in the local language, Dagbani. Surveys were back-translated into English to ensure translation accuracy. The baseline survey and 9 rounds of panel surveys were collected electronically using Open Data Kit (ODK) running on tablets. The lab data was collected on paper and later entered by data entry staff using ODK. All data was imported into Stata 12 for analysis.

Measurement

The measure of time preferences was elicited using a multiple price list. This method of measuring time preferences has been widely used in the economics literature [75]. Multiple price lists ask respondents to choose between payments of differing amounts paid at two points in time, trading off between more money at a later time and less money at an earlier time. This series of choices is intended to elicit the respondent's preferences over time and money: how much long is the respondent willing to wait for an increased payout? This allows us to infer a time discounting rate which describes the "exchange rate" between time and money. This dimension of individual preference is then assessed as it relates to choices that involve a dimension of time trade-off, such as investment decisions or, in this case, preventive health behaviors. Multiple price lists have several advantages, including relative ease of implementation and transparency to the research subject, but they also are potentially subject to bias when assuming a linear utility function [76].

The survey asked respondents about trade-offs over four time horizons: today vs. tomorrow and two days from today vs. 5, 9, and 30 days from today. There were high rates of non-response for the last two time horizons, so in this analysis uses data from two time horizons: today and tomorrow or 2 days from now and 5 days from now. Respondents were asked a series of questions about whether they preferred to receive some amount of money at a given point in time or a larger amount at a later date. The first option decreases in value while the later option remains the same through all the questions. Preferences are measured by finding an indifference point based on where a respondent switches from choosing the smaller amount sooner and opts for the larger amount with a longer delay. Using this point and the associated tradeoff, the discount rate can be calculated. An indicator variable for present bias is generated by comparing discount rates over the two time horizons. (See Appendix A for the full survey module and calculation of discount rates.)

The module was designed using real stakes (money) to incentivize respondents to think critically and answer according to their true preferences.(There is some debate over whether it is necessary to incentivize such questions with real stakes to elicit “true" preferences, see [77].) In the instructions, respondents were told that one of their choices from the 29-item module would be randomly selected to be paid out at the appropriate time horizon. Once the lab module was complete, respondents blindly drew from an envelope and were paid according to the number they drew. If they drew a number for which their choice was “today", they were paid in cash at the end of the survey. If their choice was on a future day, they were given a receipt specifying the amount and the day it was to be paid, and a member of the survey team was sent to pay out their money (also in cash) on the appropriate day. To gain the respondents’ trust and ensure that they believed we would return with their payment, we issued receipts for the amount owed that included information about the date of payment and how to contact us if we did not find them on the appointed day.

Because of working in a low-education population, the survey included several examples to allow respondents the time to ask questions and to ensure that they surveyors offered sufficient explanations to all respondents. The module also included a question where one choice should dominate the other (more money today vs less money in the future) to identify respondents who either did not understand the exercise at all or who were perhaps answering with some other reasoning that would make it hard to interpret their responses. Some respondents jumped back and forth between the earlier and the later points in time, which also prevents calculating their discount rates. To include these respondents who did not have a calculable discount rate in our analysis, a robustness check that includes an indicator variable for "no time preference value" was performed.

To measure salient health events, respondents were asked about instances of missing work due to an illness, which identifies relatively severe illness. Respondents indicated whether they had missed work due to illness in the baseline survey, and were asked the same question in the panel surveys and additionally were asked about missing work due to a child's illness and whether any children had missed school due to illness. The recall period was the past week (in the baseline survey) or the past twelve days (panel data).

The dependent variables are spending on malaria prevention and bednet utilization. Spending on malaria prevention was measured at baseline. Respondents were prompted about how much, if any, they had spent on common malaria prevention items (ITN, mosquito coils, mosquito repellent, insecticide spray for the house) in the prior week. This measures what respondents purchased, not whether they actually used the items, but it may elicit more truthful responses because it was embedded in a set of questions focused on spending patterns instead of health, which may subject respondents to social desirability bias. Bednet utilization was measured in 9 rounds of panel surveys. Respondents were asked whether their youngest child slept under a net the previous night (for all respondents with children under 10 years old.) Utilization is a measure of an actual preventive behavior, but has the weakness of being self-reported and only measuring the previous night's bednet use. To measure beliefs about malaria risk, respondents were instructed to imagine 10 children in their neighborhood and asked how many they thought would fall sick with malaria over the following day and the following week. In a review of several methods for eliciting probabilistic expectations, Delavande, Giné and McKenzie [78] find that even respondents in low-literacy environments understand key aspects of probability and that such questions predict behavior, which is also true in our sample where only 2.5% of respondents reported lower probabilities over the week compared to the next day. This question was measured in the 9 rounds of panel surveys.

Analysis

I first explored the distributions of the exposures and outcomes, and generated descriptive statistics of the study sample. I conducted three series of analyses, one for each of the primary outcomes and one mechanism analysis.

The first analysis uses the cross-section from the baseline survey data to assess the association between time preferences, salience, and spending on malaria prevention. The primary outcome, spending on malaria prevention, was measured both as a binary (any spending) and as a continuous (total amount spent). Linear regression models were used for both the binary and continuous outcomes. In sensitivity analyses I include a range of socio-demographic characteristics as controls.

$$Spending_i = \alpha_i + \beta_1 DiscountRate_i + \beta_2 PresentBiased_i + \beta_3 Illness_i + \gamma X_i + \varepsilon_i \quad (3.1)$$

Where $Spending_i$ is spending on malaria prevention (either a binary variable indicating any spending or a continuous variable), $DiscountRate_i$ is the discount rate, $PresentBiased_i$ is an indicator equal to 1 if the individual is present biased, $Illness_i$ is an indicator equal to 1 if the individual or their dependent experienced an illness episode in the past week, and X_i is a vector of controls that includes age, sex, marital status, number of dependent children, coverage under Ghana's national health insurance, weekly business earnings, and total amount of savings.

In the second set of analyses, I use the panel survey data to evaluate the association between time preferences, salience, and bednet utilization. I use a linear model, where standard errors are clustered at the individual level to account for correlation among the 9 survey rounds. I also use an individual fixed effects model to compare variation within respondents over the survey rounds. Individual-level fixed effects are used to control for respondent-specific factors, such as high malaria transmission in the respondents' immediate neighborhood, which may otherwise confound the relationship between illness episodes and bednet utilization.

Panel model:

$$Bednet_{it} = \alpha_i + \beta_1 DiscountRate_i + \beta_2 PresentBiased_i + \beta_3 Illness_{it} + \beta_4 X_i + \beta_5 R_t + \varepsilon_i + \eta_{it} \quad (3.2)$$

Where $Bednet_{it}$ is bednet utilization (binary), $DiscountRate_i$ is the discount rate, $PresentBiased_i$

is an indicator equal to 1 if the individual is present biased, $Illness_i$ is an indicator equal to 1 if the individual or their dependent experienced an illness episode in the past week, X_i is a vector of controls (same as in model 1), R_t are survey round fixed effects, ε_i is an individual-level error term and η_{it} is the individual-survey wave error.

Fixed effects model:

$$Bednet_{it} = \alpha_i + \beta_1 Illness_{it} + \beta_2 R_t + \beta_3 I_i + \varepsilon_i t \tag{3.3}$$

Where I_i are individual fixed effects and the remaining variables are the same as in the previous model.

For the mechanism analysis I use the panel data to assess the association between an exposure, health shocks, and beliefs about malaria risk. I use a linear model with controls and standard errors clustered at the individual level, as well as an individual fixed effects model.

Panel model:

$$Beliefs_{it} = \alpha_i + \beta_1 Illness_{it} + \beta_2 X_i + \beta_3 R_t + \varepsilon_i + \eta_{it} \tag{3.4}$$

Where $Beliefs_{it}$ is the malaria illness risk, $Illness_i$ is an indicator equal to 1 if the individual or their dependent experienced an illness episode in the past week, X_i is a vector of controls (same as in model 1), R_t are survey round fixed effects, ε_i is an individual-level error term and η_{it} is the error term.

Fixed effects model:

$$Beliefs_{it} = \alpha_i + \beta_1 Illness_{it} + \beta_2 R_t + \beta_3 I_i + \varepsilon_{it} \tag{3.5}$$

Where I_i are individual fixed effects and the remaining variables are the same as in the previous model.

Results

Descriptive statistics

Summary statistics are in reported in table 3.1. At baseline, respondents were asked whether they had spent any money on their own health or the health of their dependents in the past week. Out of pocket expenditure for health is common, and in this sample spending on health was very high (12 USD in the past week). Approximately half of that expenditure occurred at hospitals, and 43% of respondents spent money at a drug shop. Small drug shops and pharmacies are a common first point of care for a variety of illnesses, including malaria. 9% of respondents reported spending money due to an illness they reported as malaria, and of those reporting malaria 80% visited a drug shop for treatment while 30% visited a hospital or clinic. Slightly more than half of respondents report spending on malaria prevention (55%) but the amounts are low compared to curative care, on average 1.6 GHS (0.64 USD). This represents approximately 1.6% of median weekly earnings (100 GHS). Mosquito coils were the most commonly purchased item with 45% of households spending an average of 0.61

Table 3.1: Baseline descriptive statistics

Variable	Mean (or %)	SD	% with any
Female	80%		
Age	39.98	12.39	
Married	80%		
Weekly profit	333.83	668.25	
Number of dependent children	4.75	3.6	94%
Has active NHIS card	40%		
Spending on malaria prevention (total)	1.60	3.13	55%
Spending: Bednet	0.21	1.62	2%
Spending: Mosquito repellent	0.13	0.65	4%
Spending: Mosquito coils	0.61	0.88	45%
Spending: Insecticide for house	0.65	2.03	12%
Daily discount rate (today vs tmo)	0.944	0.095	
Daily discount rate (2 days vs 5 days)	0.971	0.041	
Present biased	37%		
Cannot calculate present bias	10%		
Days of missed work due to own illness	1.5		57%
Days of missed work due to child illness	0.70		41%
Days child missed school due to illness	0.80		45%
# children sick w/ malaria (day)	2.6	1.85	
# children sick w/ malaria (week)	4.27	2.54	
Bednet use by youngest child (previous night)	27%		

GHS on coils. While only 12% of households purchased insecticide spray, average spending was slightly higher, at 0.65 GHS.

Respondents reported higher numbers of children who would get malaria in the next week (4.27) relative to the next day (2.6) across all survey rounds. This measure is analogous to an incidence rate and therefore the estimates are very high. If the respondent had a child under the age of 10 living in the household (94% did) we asked whether the youngest child slept under a bednet the previous night. Bednet use was low, at 27% on average for all survey rounds and no individual round higher than 50%.

Illness events were common, with 56% of the sample missing at least one day of work due to illness over the 9 survey waves, 40% missing work due to a child's illness, and 45% reporting a child that missed a day of school. See appendix B for further detail.

Daily discount rates over the 2 days vs. 5 days horizon were higher (more patient, mean of 0.971) than the rate in the today vs. tomorrow horizon (mean of 0.944). A daily discount rate of 0.971 means that an individual's consumption one day later is worth 0.971 of the current value. 37% of the sample is present biased, meaning that they exhibit more impatience in time horizons that include today than in future ones. Table 3.5 shows differences in discount rates across three variables in the finance domain to establish that the time preference measurement is meaningful. (See appendix A for further description of these variables.)

Results: Health spending

Table 3.2 presents results from the first set of analyses of the association between time preferences, illness shocks, and spending on malaria prevention. There is no association between discount rates and spending, nor present bias and spending (see appendix B for full results.)

An illness shock in the past week is a large and significant predictor preventive spending. It is associated with 15 percentage points ($p<0.01$) higher probability of any spending (for malaria prevention) and a 0.93 cedi ($p<0.05$) higher spending on malaria prevention. This represents nearly a 30% increase in the probability of buying any malaria prevention, and a 2/3 increase in the average amount spent on malaria prevention. Recall periods overlap exactly meaning that the higher spending on prevention is happening in the same 2-week

Table 3.2: OLS estimates of spending on malaria prevention

	(1)	(2)	(3)	(4)
	Outcome:			
	Any spending on malaria prevention		Total spending on malaria prevention	
Daily discount rate (2 vs 5 day horizon)	0.04	0.07	-0.30	-0.43
	(0.083)	(0.105)	(0.486)	(0.506)
Present biased		0.04		0.04
		(0.058)		(0.250)
Missed work due to illness	0.14***	0.15**	1.20***	0.93**
	(0.052)	(0.060)	(0.411)	(0.391)
Constant	0.10	0.13	-0.88	0.11
	(0.165)	(0.196)	(0.997)	(1.146)
Observations	494	390	494	390
R-squared	0.028	0.029	0.063	0.056

All models include controls for gender, age, marital status, number of dependents, health insurance coverage, weekly business profit, bank account and savings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

interval as the illness episode.

Results: Bednet utilization

Table 3.3 presents results from the analysis of the association between time preferences, health shocks, and ITN utilization over four rounds of panel surveys. As with the spending results, time preferences are not a significant predictor of bednet utilization. In table 3.3, column 3 shows that missing work due to child illness is associated with 8pp higher bednet use ($p<0.10$) but the effect does not persist when controlling for individual-level characteristics by including individual fixed effects. The results for time preferences and health shocks are unchanged by the inclusion of individual-level controls (see appendix B for full results.)

Table 3.3: Bednet utilization

	(1)	(2)	(3)	(4)
	Did your youngest child sleep under a bednet last night?			
Discount rate (2 vs 5 day horizon)	0.01	0.01	0.00	
	(0.083)	(0.103)	(0.103)	
Present biased		0.01	0.01	
		(0.057)	(0.057)	
Miss work due to illness			0.08*	-0.00
			(0.047)	(0.024)
Child miss school			-0.07	-0.01
			(0.058)	(0.037)
Miss work due to child illness			0.09	-0.03
			(0.064)	(0.035)
Constant	0.45***	0.44***	0.43***	0.45***
	(0.067)	(0.097)	(0.097)	(0.014)
Survey round FE	Y	Y	Y	Y
Individual FE	N	N	N	Y
Observations	1,329	1,032	1,028	1,638
R^2	0.003	0.003	0.009	0.004
Number of respondent_id				508

*** p<0.01, ** p<0.05, * p<0.1

All models use data from the last 4 rounds (rainy season).

Robust standard errors in parentheses

Mechanisms: Malaria beliefs

Table 3.4 presents results from the analysis of malaria beliefs. Missing work due to a child's illness strongly predicts beliefs about malaria risk. Respondents whose children were ill in the previous 12 days report a subjective malaria risk assessment that is 0.5 children higher ($p<0.01$), equivalent to a 17% increase in the expectation that a child will fall ill with malaria. The relationship persists when including individual-level fixed effects, which control for any

Table 3.4: Malaria risk beliefs

	(1)	(2)
	Of 10 children in your neighborhood how many will fall sick with malaria in the next day?	
Missed work due to illness	0.10	0.02
	(0.080)	(0.081)
Child miss school	-0.02	-0.03
	(0.123)	(0.110)
Missed work due to child illness	0.50***	0.37***
	(0.130)	(0.118)
Constant	2.43***	2.67***
	(0.127)	(0.110)
Survey round FE	Y	Y
Individual FE	N	Y
Dependent variable mean	2.86	2.86
Observations	3,438	3,438
R-squared	0.015	0.012
Number of respondent_id		576

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

potential confounding due to respondent-specific factors, such as high malaria transmission in the respondents' immediate neighborhood. See appendix B for full results.

Discussion

This study assessed the associations between time preferences, salience of illness episodes, and malaria prevention in northern Ghana. I found that time preferences were not associated with spending on malaria prevention; neither the daily discount rate nor an indicator variable for present bias were associated with spending on malaria prevention. In contrast, recent salient health shocks were associated with a 15pp ($p < 0.01$) higher likelihood of spending on malaria prevention, and a 0.93 ($p < 0.05$) GHS higher level of spending on malaria prevention. Neither time preferences nor health shocks were associated with bednet utilization, after the inclusion of individual-level fixed effects that control for confounding by individual characteristics. Respondents who had a health shock reported on average higher expectations of malaria risk.

In this study I find relatively low levels of malaria prevention, which is in line with overall trends in Ghana. Since 2014 there have been large increases in free bednet distribution in Ghana's northern region, where 84% of households now have at least one ITN [71]. However utilization was much lower: only 42% of people reported using a net the previous night [71]. In our study, 27% of respondents report that the youngest child in their household used a bednet the previous night, in spite of very high perceived malaria risk. This may be attributable to the particular features of prevention, which make preventive health behaviors susceptible to present bias and inattention, leading people to fall short of their own intentions to engage in prevention. Malaria is an important case because of its large disease burden, accessible and low-cost prevention technology, and moderate (or arguably low) rates of ITN use.

Our findings can be benchmarked against similar studies of preventive spending for malaria, such as a 2012 study of women in Accra [79], which found that women spent on average 13.3 USD per illness episode, and had one illness every three months. Other features of treatment seeking were similar to our sample, with women using two drugs to treat each illness on average and choosing drug shops or pharmacies as their first point of care. The 2014 Ghana Demographic and Health Survey (DHS) reports that, among urban households, health accounted for 1.4% of household expenditure and 2% for the lowest income quintile [80]. In our study, 12 USD per week is equivalent to approximately 8% of the respondent's (not the household's) weekly earnings. Because of differing recall periods and question formulations, and collecting data on only the respondents' earnings rather than household income and expenditure, it is difficult to directly compare health spending across these studies. Our sample seems to report higher levels of spending, which may be due to the short recall period in our survey or to other factors like disease burden, insurance coverage, and patterns of care seeking.

This study did not find an association between time preferences or present bias and malaria prevention, in contrast with similar studies that find an association between time preferences and health [55, 56, 57]. It did find that there was a significant association between recent illness episodes and preventive spending. One possible explanation is that the illness events focused attention on health, which lead to an increase in preventive spending intended to avoid future illnesses. This could happen because individuals who experience an illness subsequently pay more attention to health which could lead directly to prevention, or it could cause a heightened sense of risk, true or perceived, which then leads to increased prevention. Both effects could also occur in combination.

In this study I had the opportunity to measure how beliefs evolve over time, and found higher rates of subjective risk of malaria infection after an instance of illness. This is consistent with a model where salient illness episodes focus attention on health risks, although we cannot distinguish these explanations using this dataset. Similar findings from the United States show increases in vaccination rates immediately following highly publicized disease outbreaks [81]. Taken together, these findings offer support for a model of low engagement in malaria prevention that is driven by inattention rather than present bias. This interpretation has implications for what public health practitioners and policy makers should expect if interventions are successful in increasing malaria prevention: if better prevention and treatment leads to fewer illnesses, individuals may stop being wary of the disease, which could foil efforts to eradicate it.

This study has strengths and limitations. An important strength is the dataset, which contain repeated observations allowing for individual fixed effects analysis, which solves many (although not all) issues of unmeasured confounding. In addition, by nesting these questions into a study unrelated to health or malaria, we had the opportunity to ask about ITN utilization and purchasing of prevention products in a context that is less likely to produce social desirability bias, compared to similar survey questions implemented in a health-focused survey or after an intervention targeting malaria. However, these outcome measures are still limited by being self-reports and were limited in scope to focus only on two aspects of malaria prevention. The data collection structure also limited the covariates that were measured and the small sample precludes potentially informative subgroup analyses. Another limitation is that measures of time preference are borrowed from economics and were framed in the financial domain, but there are some outstanding questions in the literature about how preferences translate into the health domain [82]. Finally, the estimates are associational, not causal, due to the non-random nature of preferences and illness events.

Conclusion

In this study, I evaluated the association between time preferences, salient illness events, and malaria prevention, and found evidence consistent with a model of inattention. Preventive health behaviors nearly always involve taking an action in the present to avoid a potential future cost. In an environment where attention is limited, a salient illness may focus attention on health and increase preventive behaviors. These findings contribute to the evidence that behavioral economic models of decision making may be useful in understanding health behaviors, especially where traditional approaches to public health have not been particularly effective.

Appendix A: Measurement of Time Preferences

Of 636 baseline surveys, 599 contained completed time preference sections. To calculate the discount rate, respondents must switch from one column to the other. Normally they start off choosing money today (7.5 today vs 8 tomorrow) and should change to the tomorrow column as the money offered today gets smaller. The point at which they switch gives us the bound for an indifference point and allows us to calculate a discount rate. For this calculation to be feasible we need people to (a) begin in the left column and (b) change to the right column exactly once. Those who switch back and forth many times don't give us enough information to calculate their rate so they are coded as missing. Others just stay in one column the whole time, so we can characterize their preferences even if they don't allow us to calculate an exact rate: If someone stays always in the left (today) column they are very impatient, i.e. they highly discount the future. If someone starts in the right (tomorrow column) they are very patient, i.e. they do not heavily discount the future. For example, a respondent who is always in the today column for today/tmo time horizon but then changes for the 2/5 day horizon should be coded as present biased.

If I switch at line 3: I prefer 8 tmo over 6.5 today so: 6.5=delta*8
For those always in col 0: assume they would switch at 0, take halfway point between that and 4.5

			Exponent on delta (1/(T2-T1))		
switching point	Column 0 amt in T1	Column 1 amt in T2	today/tmo	2 days/5 days	2 days/9 days
			1	0.3333333333	0.1428571429
1 (always in col 1)	7.5	8	1	1	1
2	7	8	0.875	0.9565	0.9811
3	6.5	8	0.8125	0.9331	0.9708
4	6	8	0.75	0.9086	0.9597
5	5.5	8	0.6875	0.8826	0.9479
6	5	8	0.625	0.8550	0.9351
7	4.5	8	0.5625	0.8255	0.9211
always in col 0	0	8	0	0.0000	0.0000
Assume halfway poin	0	8	0.28125	0.4127409061	0.4605462203

* lower number here is less patient, 1 means you do not discount the future at all

Figure 3.2: Calculation of discount rate based on survey response

Figure 3.3: Comparison of time discounting over two time horizons

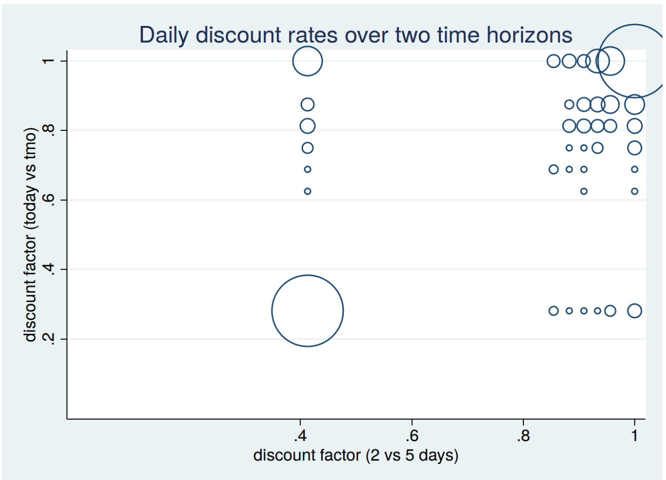

Figure 3.4: Daily discount rates, circle proportional to sample size

Table 3.5: Comparison of discount rates across groups

Variable	Mean of discount rate, today vs. tmo		
	No	Yes	Difference
Regularly meets savings goals?	0.77	0.71	2.03**
	(0.02)	(0.02)	
Made any business investment (past 3m)?	0.72	0.75	-0.87
	(0.02)	(0.03)	
Has savings account in bank?	0.75	0.68	2.36**
	(0.02)	(0.03)	
Variable	**Mean of discount rate, 2 vs. 5 days**		
	No	Yes	Difference
Regularly meets savings goals?	0.78	0.73	1.80*
	(0.02)	(0.02)	
Made any business investment (past 3m)?	0.74	0.76	-0.54
	(0.02)	(0.02)	
Has savings account in bank?	0.77	0.71	2.15**
	(0.01)	(0.02)	

INCENTIVES TO SAVE – Baseline Lab Session **Respondent ID: __________**

SURVEYOR:

1. *Do the **F1, F2, F9, F10** as a trial.*
2. *Make sure respondent understands the activity. Ask the respondent to explain the process to you, if they understand it.*
3. *Once you have confirmed that the respondent understands; tell respondent you will do this again and that now their decisions will be implemented after the draw from the bowl.*

		GHS		Tick if chosen		GHS		Tick if chosen
F1	Would you prefer	**7.50p**	today		**OR**	**8**	tomorrow	
F2	Would you prefer	**7**	today		**OR**	**8**	tomorrow	
F3	Would you prefer	**6.50p**	today		**OR**	**8**	tomorrow	
F4	Would you prefer	**6**	today		**OR**	**8**	tomorrow	
F5	Would you prefer	**5.50p**	today		**OR**	**8**	tomorrow	
F6	Would you prefer	**5**	today		**OR**	**8**	tomorrow	
F7	Would you prefer	**4.50p**	today		**OR**	**8**	tomorrow	
F8	Would you prefer	**9**	today		**OR**	**8**	tomorrow	
F9	Would you prefer	**7.50p**	2 days from today		**OR**	**8**	5 days from today	
F10	Would you prefer	**7**	2 days from today		**OR**	**8**	5 days from today	
F11	Would you prefer	**6.50p**	2 days from today		**OR**	**8**	5 days from today	
F12	Would you prefer	**6**	2 days from today		**OR**	**8**	5 days from today	
F13	Would you prefer	**5.50p**	2 days from today		**OR**	**8**	5 days from today	
F14	Would you prefer	**5**	2 days from today		**OR**	**8**	5 days from today	
F15	Would you prefer	**4.50p**	2 days from today		**OR**	**8**	5 days from today	
F16	Would you prefer	**7.50p**	2 days from today		**OR**	**8**	9 days from today	
F17	Would you prefer	**7**	2 days from today		**OR**	**8**	9 days from today	
F18	Would you prefer	**6.50p**	2 days from today		**OR**	**8**	9 days from today	
F19	Would you prefer	**6**	2 days from today		**OR**	**8**	9 days from today	
F20	Would you prefer	**5.50p**	2 days from today		**OR**	**8**	9 days from today	
F21	Would you prefer	**5**	2 days from today		**OR**	**8**	9 days from today	
F22	Would you prefer	**4.50p**	2 days from today		**OR**	**8**	9 days from today	
F23	Would you prefer	**7.50p**	2 days from today		**OR**	**8**	30 days from today	
F24	Would you prefer	**7**	2 days from today		**OR**	**8**	30 days from today	
F25	Would you prefer	**6.50p**	2 days from today		**OR**	**8**	30 days from today	
F26	Would you prefer	**6**	2 days from today		**OR**	**8**	30 days from today	
F27	Would you prefer	**5.50p**	2 days from today		**OR**	**8**	30 days from today	
F28	Would you prefer	**5**	2 days from today		**OR**	**8**	30 days from today	
F29	Would you prefer	**4.50p**	2 days from today		**OR**	**8**	30 days from today	

Figure 3.5: Survey module used to elicit time preferences

Appendix B: Results

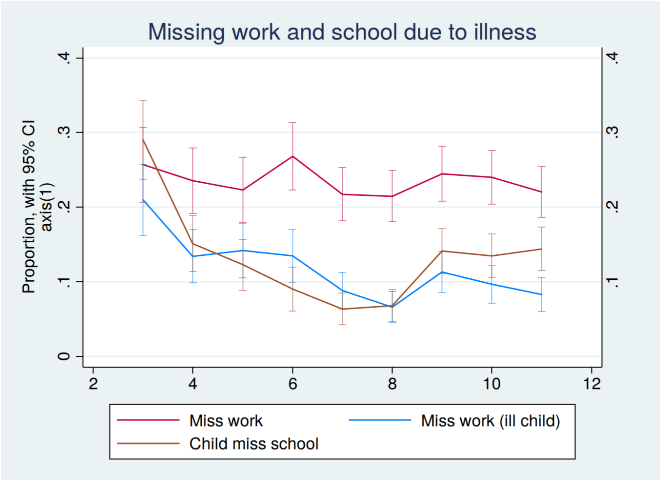

Figure 3.6: Proportion of the sample missing work and school due to illness, by survey round

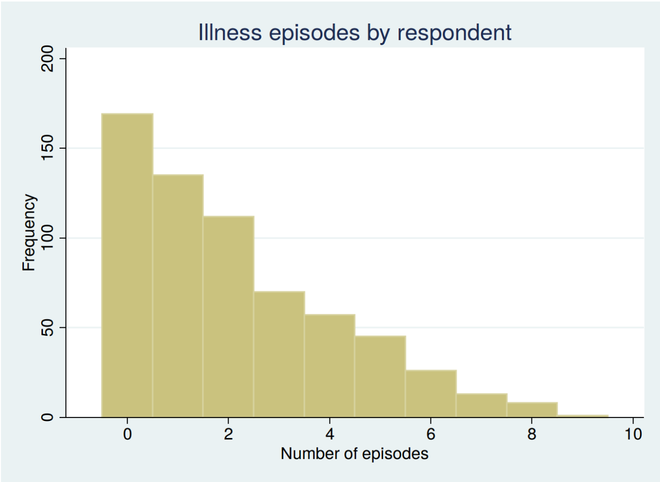

Figure 3.7: Distribution of total illness episodes over study period, by respondent

Table 3.6: Full results: OLS estimates of association between present bias, illness episode, and any malaria prevention spending

	(1)	(2)	(3)	(4)
Outcome:	Any spending on malaria prevention			
Daily discount rate (2 days vs. 5 days)	0.05	0.09	0.07	0.03
	(0.082)	(0.103)	(0.105)	(0.083)
Present biased		0.05	0.04	
		(0.057)	(0.058)	
Missed work due to illness			0.15**	0.14***
			(0.060)	(0.052)
Female			0.00	0.04
			(0.067)	(0.061)
Age			-0.00	0.00
			(0.002)	(0.002)
Married			0.05	0.07
			(0.068)	(0.059)
Number of dependents			0.00	0.00
			(0.007)	(0.006)
Has NHIS			-0.02	-0.02
			(0.058)	(0.051)
Weekly profit			0.03	0.03*
			(0.020)	(0.018)
Has formal savings account			0.02	0.03
			(0.061)	(0.055)
Total current savings			-0.00	-0.00
			(0.000)	(0.000)
Present bias measure missing				0.05
				(0.055)
Constant	0.51***	0.45***	0.13	0.09
	(0.066)	(0.098)	(0.196)	(0.165)
Observations	494	390	390	494
R^2	0.001	0.003	0.029	0.030

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Full results: OLS estimates of association between present bias, illness episode, and total amount of malaria prevention spending

	(1)	(2)	(3)	(4)
Outcome:	Total spending on malaria prevention			
Daily discount rate (2 days vs. 5 days)	-0.43	-0.52	-0.43	-0.40
	(0.490)	(0.488)	(0.506)	(0.477)
Present biased		0.10	0.04	
		(0.251)	(0.250)	
Missed work due to illness			0.93**	1.18***
			(0.391)	(0.404)
Female			-0.61	-0.59
			(0.439)	(0.380)
Age			-0.01	-0.01
			(0.009)	(0.009)
Married			0.13	0.33
			(0.297)	(0.250)
Number of dependents			0.01	0.01
			(0.027)	(0.028)
Has NHIS			0.15	0.31
			(0.305)	(0.269)
Weekly profit			0.20	0.26**
			(0.130)	(0.110)
Has formal savings account			0.39	0.27
			(0.396)	(0.359)
Total current savings			0.00	0.00
			(0.000)	(0.000)
Present bias measure missing				0.49
				(0.357)
Constant	1.89***	1.79***	0.11	-0.92
	(0.391)	(0.441)	(1.146)	(1.011)
Observations	494	390	390	494
R^2	0.002	0.004	0.056	0.067

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Full results: Panel estimates of association between present bias, illness episode, and bednet use, in dry season

	Bednet use: dry season				
Discount rate (2 vs 5 day)	-0.03	0.01	0.01	0.03	
	(0.094)	(0.122)	(0.123)	(0.125)	
Present biased		0.01	0.00	0.01	
		(0.068)	(0.068)	(0.069)	
Missed work (ill health)			-0.02	-0.03	0.02
			(0.061)	(0.063)	(0.066)
Child miss school			-0.03	-0.05	-0.11
			(0.111)	(0.112)	(0.095)
Miss work (child ill)			0.12	0.13	0.05
			(0.111)	(0.112)	(0.093)
Female				-0.05	
				(0.070)	
Age				0.00	
				(0.003)	
Married				0.00	
				(0.093)	
Number of dependents				-0.00	
				(0.006)	
Weekly earnings				0.00	
				(0.000)	
Has formal savings account				-0.11*	
				(0.066)	
Constant	0.45***	0.43***	0.42***	0.47**	0.45***
	(0.078)	(0.116)	(0.117)	(0.230)	(0.078)
Individual FE	N	N	N	N	Y
Observations	541	426	423	423	557
R^2	0.001	0.000	0.004	0.024	0.009
F-stat for all illnesses	.	.	0.42	0.51	0.58

All models include survey round fixed effects. The last row displays the test statistic for F-tests of joint significance of all three illness variables. Standard errors (in parentheses) clustered at individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Full results: Panel estimates of association between present bias, illness episode, and bednet use, in rainy season

	Bednet use: rainy season				
Discount rate (2 vs 5 day)	0.01	0.01	0.00	0.02	
	(0.083)	(0.103)	(0.103)	(0.105)	
Present biased		0.01	0.01	0.02	
		(0.057)	(0.057)	(0.057)	
Missed work (ill health)			0.08*	0.08*	-0.00
			(0.047)	(0.047)	(0.026)
Child miss school			-0.07	-0.07	-0.03
			(0.058)	(0.057)	(0.034)
Miss work (child ill)			0.09	0.08	-0.03
			(0.064)	(0.063)	(0.040)
Female				-0.01	
				(0.061)	
Age				0.00	
				(0.003)	
Married				0.06	
				(0.071)	
Number of dependents				-0.00	
				(0.006)	
Weekly earnings				0.00***	
				(0.000)	
Has formal savings account				-0.06	
				(0.059)	
Constant	0.45***	0.44***	0.43***	0.30	0.45***
	(0.067)	(0.097)	(0.097)	(0.185)	(0.032)
Individual FE	N	N	N	N	Y
Observations	1,329	1,032	1,028	1,028	1,428
R^2	0.003	0.003	0.009	0.028	0.007
F-stat for all illnesses	.	.	1.67	1.79	0.52

All models include survey round fixed effects. The last row displays the test statistic for F-tests of joint significance of all three illness variables. Standard errors (in parentheses) clustered at individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Full results: Panel estimates association between illness episode and malaria risk beliefs

	Panel				With individual FE	
	Of 10 children, how many will get malaria in next					
	Day		Week		Day	Week
Missed work due to illness	0.10	0.10	0.04	0.06	0.02	0.02
	(0.088)	(0.089)	(0.123)	(0.123)	(0.087)	(0.107)
Child miss school	-0.02	-0.02	0.04	0.04	-0.03	-0.02
	(0.141)	(0.137)	(0.180)	(0.176)	(0.114)	(0.151)
Miss work due to child illness	0.50***	0.50***	0.39**	0.39**	0.37***	0.23
	(0.138)	(0.137)	(0.179)	(0.178)	(0.111)	(0.159)
Female		0.09		-0.06		
		(0.149)		(0.211)		
Age		0.00		0.00		
		(0.005)		(0.007)		
Married		0.08		0.12		
		(0.141)		(0.198)		
Number of dependents		0.00		-0.01		
		(0.021)		(0.028)		
Registered with NHIS		-0.08		-0.15		
		(0.128)		(0.185)		
Weekly earnings		0.00		0.03		
		(0.044)		(0.061)		
Has formal savings account		0.05		0.03		
		(0.151)		(0.199)		
Constant	2.43***	2.16***	4.09***	3.93***	2.67***	4.28***
	(0.125)	(0.430)	(0.170)	(0.601)	(0.118)	(0.151)
Observations	3,438	3,438	3,425	3,425	3,438	3,425
R-squared	0.015	0.016	0.017	0.019	0.012	0.020
F-stat for all illnesses	6.26***	6.03***	4.87***	2.53**	2.54**	0.99

All models include survey round fixed effects. The last row displays the test statistic for F-tests of joint significance of all three illness variables.
Standard errors (in parentheses) clustered at individual level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4

Making it personal: The effect of goal-oriented personalized reminders on gym attendance

Introduction

In the U.S., rates of physical activity are low [83], with only 1 in 5 adults meeting federal guidelines for physical activity and no measurable improvement between 2009-2013 [9]. Low rates of exercise are an important contributor to the chronic disease burden [84]; they are associated with the high rates of diabetes and poor cardiovascular health in the U.S. Numerous federal and state policies aim to increase exercise as a means of achieving improvements in chronic disease outcomes [9]. Yet, even individuals who desire and intend to exercise have difficulty doing so. After enrolling in year-round contracts in gym clubs, many members fail to attend regularly enough to justify their initial financial commitment. Acland and Levy [11] provide direct evidence of over-prediction in the frequency of workouts, indicating that people intend to exercise more frequently but do not follow through. Signing up for gym memberships is a strong signal that they intend to exercise, and individuals are even willing to pay for commitment devices [85] which demonstrates that they are aware of their need for help in attaining their exercise goals.

There are many potential explanations for why individuals do not exercise as frequently as they want to. Time inconsistency, a behavioral bias wherein people weigh immediate costs highly and discount future benefits, is a potential explanation for the "intention-action gap" observed in gym attendance. Garon and colleagues [86] find evidence of time inconsistency among gym attendees using administrative data, and DellaVigna and Malmendier similarly find that gym members predict that they will attend at higher rates than they in fact do [87]. Charness and Gneezy [88] provide evidence that financial incentives designed to outweigh the immediate cost of exercising can increase frequency of gym attendance. Similarly, people may fail to exercise because they have many competing priorities and exercise is not the most salient issue. Interventions to increase salience may be effective at changing behaviors in domains such as saving money [60], reducing energy consumption [89], and healthy eating [90].

Incentive programs for exercise are costly to implement and their effect on attendance appears to be mostly temporary [91, 88]. Lower-cost interventions such as reminders might also help individuals to make better decisions. Reminders may highlight future benefits, thereby prompting action in time-inconsistent individuals, or may simply prompt individuals to remember to exercise.

While there are many studies demonstrating the effectiveness of reminders for a variety of health applications including medication adherence, appointment attendance, and follow-up for test results [92], there is less evidence to inform the content of reminders. A study of reminders of health information increased correct knowledge but did not translate into healthier behavior [93]. Qualitative evidence from Uganda [94] indicates that the content of reminders influences how they are received and understood by HIV patients and may be important for effectiveness. Reback and colleagues [95] compared text message content informed by three different theories of health behavior and found that their impact varied widely.

More research is needed on how to design the content of the reminders to make them effective for changing health behaviors in order to impact health outcomes. This study explores one potential way of enhancing reminders' effectiveness by leveraging behavioral biases. We compare the effect of two types of SMS reminders that encourage gym attendance: A personal goal reminder that explicitly benchmarks participants against a previously-stated goal in order to motivate them to exercise, and a simple, general reminder to exercise that contains no personalized information. We ran a randomized controlled trial on members of a gym in Montreal to evaluate the impact of a simple SMS reminder compared to a reminder containing a personal goal. We measure the effect on gym members' attendance frequency and exercise goal attainment using administrative attendance data.

Data and Methods

Sample selection and enrollment

We partnered with 12 locations of a chain of gyms in Montreal, Quebec. Members were eligible to enroll in our SMS reminder program at the time that they signed up for memberships or renewed a previous membership between January and March 2016. We included a consent form with the routine membership application paperwork that was distributed to members. They gave signed consent and recorded their phone number if they decided to participate in the reminder program. Gym employees gathered the consent forms, and the study team collected them at each gym location over the course of the enrollment period. Members were eligible for the study if they filled out the membership paperwork during the enrollment period, signed the consent form, and entered a valid mobile phone number on the consent form. We excluded individuals who did not state a weekly exercise goal on their form since the intervention is designed around that goal.

Data

Data for this study consists entirely of administrative data from the gyms. Two types of administrative data were obtained from the gyms: Data taken from the membership enrollment forms, and timestamps of gym attendance. The membership enrollment forms were collected from the gym locations by a member of the study team and entered into a database using the Open Data Kit (ODK) data entry platform on a mobile phone. The forms contain basic demographic data (age, sex), and self-reports of prior gym attendance, reasons for enrolling in a gym, attendance goals, and several questions about motivation to exercise and difficulties related to healthy behaviors in the past. We use two questions about motivation to construct a binary variable of “motivation problems", equal to 1 if respondents reported that they agree or strongly agree with either of the following statements: That it requires effort to go to the gym, and that they have had problems maintaining their gym attendance frequency. All responses were given on a Likert scale (strongly disagree to strongly agree).

Gym attendance is recorded using timestamps from gym members’ swipecards that they use to enter the gym. Timestamp data was transferred to the study team in excel spreadsheets. In addition, we tracked which participants unsubscribed from the SMS reminder program via our online SMS platform, CommCareHQ.

Design of intervention and experiment

Drawing on previous literature that demonstrates that reminders can be effective in increasing exercise, the intervention in this study is designed to enhance the salience of an exercise goal. In a meta-analysis of SMS reminder interventions, reminders were most effective for exercise and smoking cessation, and personalizing the content was found to increase effectiveness [96]. Calzolari and Nardotto [97] find that reminders cause individuals to exercise in the immediate period after receiving the reminder, providing support for the idea that a salient reminder can prompt action. John and colleagues find that non-salient financial incentives did not increase exercise substantially, but adding emails that made the incentive salient was highly effective [98]. In this study, the personal goal reminder is designed to motivate gym attendance by making goals salient and leveraging participants' loss aversion. Loss aversion describes the feature of behavior that people respond more strongly to losses than to gains of a similar magnitude [99]. The personal goal message reminds the participant of their goal, which is intended to motivate them to exercise and therefore avoid falling short of their goal and experiencing a perceived loss.

General reminder text:
Don't forget to exercise. Your [name of gym] centre is waiting for you! (STOP to unsubscribe)

Personalized goal reminder text:
Don't forget your exercise goal: X time(s) per week. Your [name of gym] centre is waiting for you! (STOP to unsubscribe)

Participants were randomized into two equal groups, a personal goal reminder group and a general reminder group. The personal goal reminder group received SMS reminders that highlighted their goal for weekly gym attendance, which they indicated on the enrollment form. The general reminder group received a reminder to go to the gym without reference to their own goal. Messages were sent in English or in French depending on the participant's preferred language.

The randomization was carried out using Stata 12. We used stratification to ensure balance on key variables, and tested the balance on other variables after randomization to ensure the groups were similar. We stratified on goal number of visits per week (less than 3, 3, more than 3), and pre-intervention attendance (over/under median number of visits). After the groups were randomized, we confirmed that the sample was balanced on a range of variables.

Table 4.1 in section 4.3 shows treatment group balance and sample characteristics.

Intervention procedures

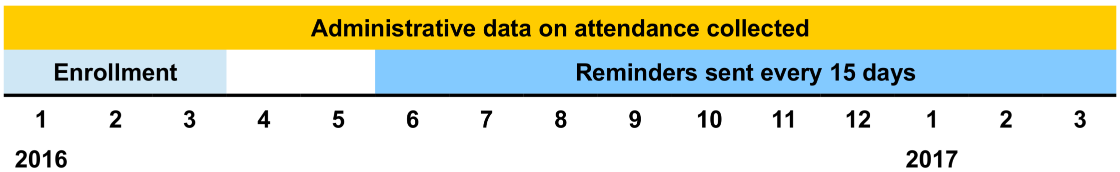

Figure 4.1: Timeline of study activities

We enrolled participants from Jan to March 2016, and sent SMS reminders every 15 days from May 30, 2016 through the end of March 2017. We began sending the reminders on May 30, 2016, and continued the intervention until March 2017. Swipecard timestamp data of all gym visits was collected throughout the study period (see figure 4.1.)

SMS reminders were sent on an automated schedule at the same time (1:30pm) every 15 days during the study period for a total of 21 messages over 10 months. Participants were able to unsubscribe from the reminders at any time by replying to a reminder message. Messages were sent using the online SMS platform provided by CommCareHQ. Through this platform, we used an international SMS server (Tropo) to send messages and receive unsubscribe replies. The messages appeared as though they were sent from a local phone number. Because the message formulation mentioned the name of the gym and the consent process occurred when participants began their gym memberships, it is likely that participants assumed the gym was sending the messages directly.

The 15-day interval was selected to balance enough reminder frequency with the risk of high proportions of respondents unsubscribing due to receiving too many messages. We chose to send them every 15 days as opposed to exactly every 2 weeks to avoid having messages fall on the same day each week. We continued to observe the attendance of those who opt-out of receiving reminder messages through the administrative data.

The project received ethics approval from the Université du Québec à Montréal.

Analysis

Outcomes

The goal was elicited as a weekly attendance goal, so the primary outcome is the probability of meeting one's goal in a given week. The outcome is coded as a binary variable, equal to 1 if the participant met (or exceeded) the weekly goal and 0 otherwise. As a secondary outcome, we measure the average number of visits per week. We also measure the probability of unsubscribing from the reminders.

Regression analyses

We estimate a model of aggregate outcomes at the end of the intervention period, in March 2017, and estimate panel models during the intervention period.

For the individual-level analysis of aggregate outcomes we use the following model:

$$y_i = \beta_0 + \beta_1 Personal_i + \beta_3 X_i + \beta_3 G_j + \epsilon_i \tag{4.1}$$

where

- y_i is the outcome: proportion of weeks goal was met, total number of gym visits, probability remaining subscribed to SMS reminders

- $Personal_i$ is a dummy variable that equals 1 if the respondent is in our Personal SMS treatment group, and equals 0 if they are in the general reminder (control) group.

- X_i is a vector of baseline characteristics, and includes the stratification variables (prior attendance and level of weekly goal) as well as other covariates (age, gender, and others)

- G_j is a gym branch fixed effect

- ϵ_i is the error term

For the panel analysis, we estimate the following model:

$$y_{it} = \beta_0 + \beta_1 Personal_i + \beta_3 X_i + \beta_3 G_j + \beta_4 P_t + \phi_{it} \tag{4.2}$$

where

- y_{it} is the outcome: goal attainment, number of gym visits, or an indicator for unsubscribing from the SMS reminders

- $Personal_i$ is a dummy variable that equals 1 if the respondent is in our Personal SMS treatment group, and equals 0 if they are in the general reminder (control) group.

- X_i is a vector of baseline characteristics, and includes the stratification variables (prior attendance and level of weekly goal)

- G_j is a gym branch fixed effect
- P_t is a period fixed effect (for each 15-day period beginning on the day a reminder was sent)
- ϕ_{it} is the error, which accounts for clustering at the individual level

Results

Sample characteristics and treatment balance

Table 4.1: Balance across treatment groups

	(1)		(2)		(3)	
	General SMS group		Personal SMS group			
	Mean	SD	Mean	SD	Difference	T-stat
Respondent's age	37.53	13.75	37.99	13.20	-0.46	(-0.32)
Respondent is female	0.35	0.48	0.39	0.49	-0.04	(-0.79)
Respondent is smoker	0.11	0.31	0.14	0.35	-0.03	(-0.93)
Pre-treatment attendance	2.35	1.07	2.26	0.98	0.09	(0.78)
Number of years with goal	2.76	3.82	2.46	3.64	0.29	(0.52)
Goal visits per week	2.99	0.79	3.01	0.92	-0.01	(-0.14)
Has motivational problems	0.75	0.06	0.71	0.06	0.05	(0.55)
Proportion with weekly goal	0.81	0.03	0.81	0.03	0.003	(0.08)
Observations	178		180		358	

Table 4.1 shows the treatment group balance across several baseline characteristics. The sample is well-balanced on all variables, including pre-intervention attendance and an index of motivational problems. The sample is less than half female and is on average 37 years old. Montreal is a city with two distinct linguistic groups and the sample is primarily French-

speaking with a small group (9%) of native English speakers.

The sample is composed of gym members who consented to participate in the study, and we can compare them to gym members who did not consent to the SMS reminder intervention but for whom we obtained de-identified attendance data. On average our sample is slightly different from the general population of new gym members. Our sample chose an exercise goal more recently than non-participants (2.62 years vs. 3.73 years, $p<0.05$). 73% of the sample reported a motivation problem, compared with 64% of those who did not consent, although the difference is not statistically significant. This provides some suggestive evidence that our sample has higher rates of self-control problems than the non-participants or is more aware of their self-control problems (or both), which may help explain why these individuals chose to participate in the reminder program.

Gym attendance goals

When our study participants signed up for gym memberships, they were asked how many times per week they intended to exercise at the gym. On average, members indicated a goal of three times per week, as shown in figure 4.2. In the period between the goal elicitation and the beginning of the intervention, gym members attended on average approximately once per week, so the goals are relatively high compared to pre-intervention attendance. In figure 4.3 we show pre-intervention weekly attendance for each goal level. Although those with higher weekly goals do attend slightly more before the intervention, the gap between pre-intervention attendance and weekly goal is large for the majority of the sample. Attendance is highest on Mondays and declines throughout the week, and the time of day is bi-modal with peaks around 11am and 5pm.

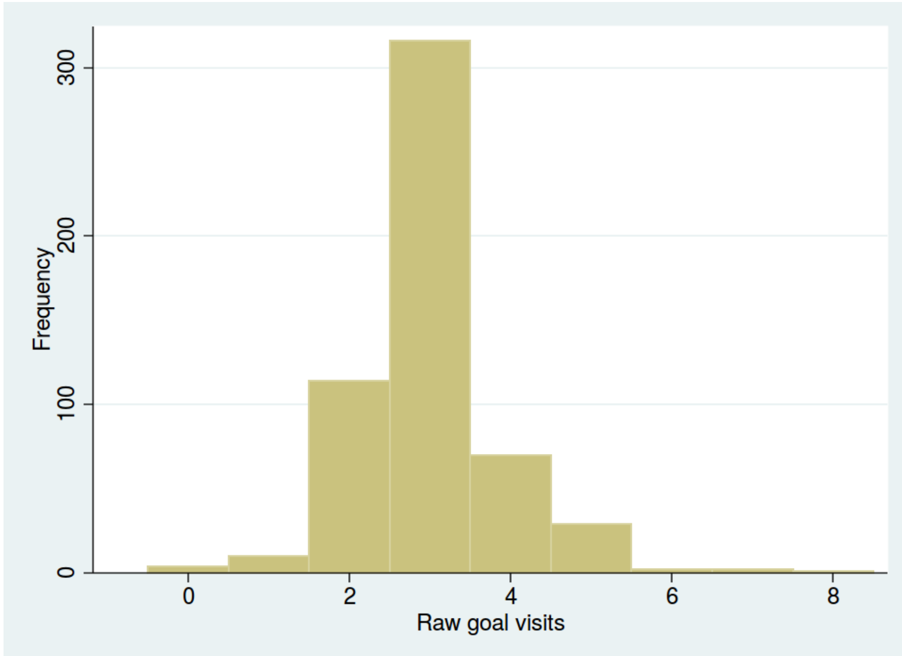

Figure 4.2: Distribution of weekly goals

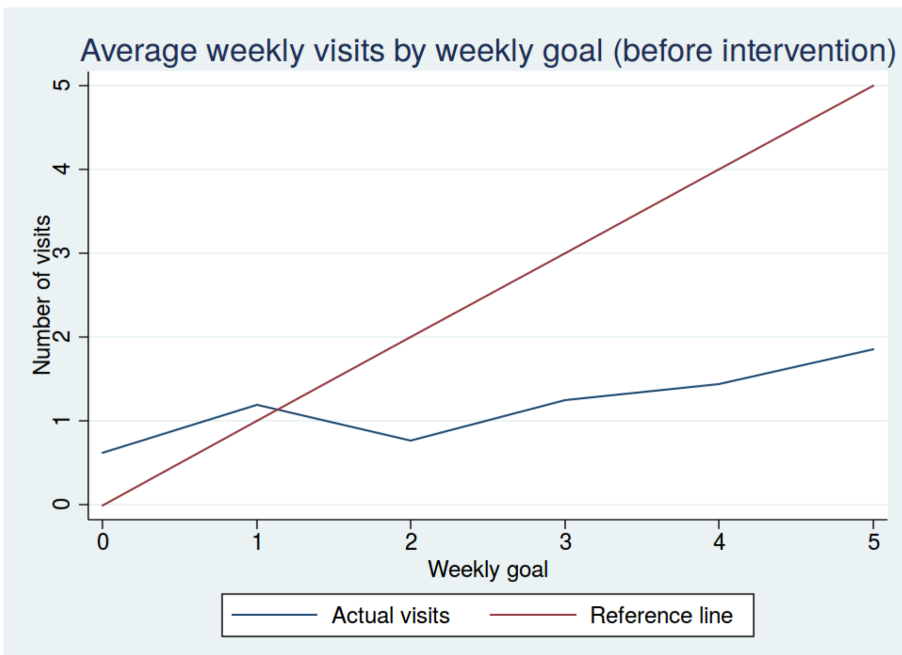

Figure 4.3: Weekly goal and mean weekly gym visits

Main results: Goal attainment and gym attendance

Table 4.2 reports the results of a panel estimation of the treatment on the probability of attaining one's goal and on the number of weekly gym visits. The personal goal reminder did not have a significant effect on either outcome. Similarly, in table 4.3 we report the aggregate outcomes, number of weeks where the goal was met and total number of gym visits during the study period. The treatment did not have a significant effect on either aggregate outcome. Appendix B reports the full results. The level of the weekly goal was associated with significantly more gym visits in both panel and aggregate models, and the indicator for motivation problems was associated with both lower rates of goal attainment and lower numbers of visits.

Table 4.2: Panel estimates: Effect of personal SMS reminder

	(1)	(2)	(3)	(4)
	Outcome:			
	Attained goal		# weekly visits	
Personal reminder	0.01	0.00	0.10	0.06
	(0.022)	(0.021)	(0.110)	(0.101)
Stratification variables	Y	Y	Y	Y
Additional controls	N	Y	N	Y
Week FE	N	Y	N	Y
Gym branch FE	N	Y	N	Y
Observations	9,864	9,828	9,864	9,828
R-squared	0.042	0.106	0.093	0.191

All models include stratification variables (number of goal visits and pre-period attendance.) Individual-level controls: Age, sex, smoker, prior gym membership experience, and indicator of motivation problems. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 4.3: Aggregate estimates: Effect of personal SMS reminder

	(1)	(2)	(3)	(4)
	Outcome:			
	# Weeks attained goal		Total visits	
Personal reminder	1.44	1.01	4.23	2.98
	(1.229)	(1.142)	(3.819)	(3.666)
Stratification variables	Y	Y	Y	Y
Additional controls	N	Y	N	Y
Gym branch FE	N	Y	N	Y
General reminder mean	8.97	8.97	26.51	26.51
Observations	291	290	291	290
R-squared	0.155	0.336	0.175	0.308

All models include stratification variables (number of goal visits and pre-period attendance.) Individual-level controls: Age, sex, smoker, prior gym membership experience, and indicator of motivation problems. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Mechanisms

In this section, we consider a few reasons why the treatment may not have had an effect on either goal attainment or exercise frequency. First, as noted in section 4.3.2, goals were set high relative to pre-intervention attendance so we investigate whether the intervention was more effective for people who set more realistic goals or who report more motivation problems. We then assess the role of program attrition to determine if systematic unsubscribing from reminders may explain the results. Lastly, we estimate a pooled analysis of both SMS reminders to see if receiving any reminder had an effect.

Were personal goal reminders discouraging?

One potential explanation for not observing an effect is that the personal goal reminders were discouraging. We assess the interaction of our treatment with the gap between pre-intervention attendance and goal, which is a measure of how realistic/attainable the goal was. We then estimate a model with an interaction between the treatment and the indicator of motivational problems, to evaluate whether those who self-report more difficulty with motivation were more impacted by the reminders. We then evaluate differential attrition by SMS group to determine whether the personal goal SMS group generated more drop-outs, which may indicate that respondents disliked that message more than the general reminder.

As discussed in section 4.3.2, the goals participants set were, on average, much higher than their baseline attendance rates. This may make the reminder about the goals discouraging rather than encouraging, and thus may explain why the intervention did not increase exercise rates. In table 4.4 we report the results of a model similar to table 4.3 but which includes an interaction term between personal goal SMS and the pre-intervention gap between weekly goal and weekly attendance (where a positive value means that the attendance exceeds the goal.)

There is no significant effect of the treatment on goal attainment (columns 1 and 2). Column 4 reports that after controlling for individual-level covariates, the personal reminder increased total gym visits by 10.67 visits over the study period ($p<0.1$), holding the level of the gap from goal at zero. The effect of the gap from goal is positive, indicating an association between exceeding one's goal and higher total gym visits as well as more weeks where the goal was attained. The interaction between the personal reminder and the gap from goal is positive, meaning that those who received the personal goal reminder experienced higher gym attendance that increases with their pre-intervention attendance relative to their goal: an increase of 7.47 visits ($p<0.1$) over the study period for every one-visit increase above the gap (or for every one-visit reduction in gap) for those in the personal reminder group, equivalent to an increase of 2/3 of a visit per month. See appendix C for the full regression results. We estimate the marginal effects for all levels of gap from goal, which indicates that that estimated effect is positive for those who exceed their goal in the pre-intervention period, as well as for those who are below their goal by a small amount (gap of -1 or -2). For those who are 3 or more visits away from their goal in the pre-intervention period, the effect goes away or even becomes negative. This suggests that the reminder was effective for those who were performing near or above their goal, but may have been discouraging for those who were far from their weekly goal.

In table 4.5 we report the results of a similar model, with an interaction between the personal goal reminder and the indicator of motivational problems. There is no significant effect of the personal reminder or the interaction between personal reminder and motivation problem, indicating that the treatment did not perform differently by baseline motivation levels. There is an association between motivation problems and both lower rates of goal attainment and fewer gym visits.

Table 4.4: Effect of SMS reminders and gap between pre-intervention attendance and goal

	(1)	(2)	(3)	(4)
	# Weeks attained goal		Total visits	
Personal reminder	1.62	1.93	8.70	10.67*
	(1.791)	(1.637)	(6.338)	(5.854)
Average weekly gap from goal	3.94***	3.70***	13.47***	13.27***
	(0.725)	(0.684)	(2.240)	(1.948)
Personal reminder X gap	0.44	1.04	4.71	7.47**
	(0.897)	(0.891)	(3.668)	(3.522)
Additional controls	N	Y	N	Y
Gym branch FE	N	Y	N	Y
General reminder mean	8.97	8.97	26.51	26.51
Observations	291	290	291	290
R-squared	0.309	0.473	0.404	0.539

All models include stratification variables (number of goal visits and pre-period attendance.) Individual-level controls: Age, sex, smoker, prior gym membership experience, and indicator of motivation problems. *** p<0.01, ** p<0.05, * p<0.1

Rates of program attrition were higher in the personal goal treatment relative to the general

Table 4.5: Effect of self-reported motivation problems

	(1)	(2)	(3)	(4)
	# Weeks attained goal		Total visits	
Personal reminder	1.65	0.70	4.68	1.84
	(1.718)	(1.673)	(5.782)	(5.725)
Has motivation problem	-5.92***	-5.74***	-15.68***	-15.60***
	(1.630)	(1.715)	(4.381)	(4.484)
Personal SMS X motivation problem	-0.49	0.97	0.44	5.18
	(2.423)	(2.373)	(7.331)	(7.095)
Additional controls	N	Y	N	Y
Gym branch FE	N	Y	N	Y
General reminder mean	8.97	8.97	26.51	26.51
Observations	291	290	291	290
R-squared	0.220	0.336	0.217	0.310

All models include stratification variables (number of goal visits and pre-period attendance.) Individual-level controls: Age, sex, smoker, prior gym membership experience, and indicator of motivation problems. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

reminder (see figure 4.4). This may indicate that participants receiving the personal goal messages disliked them more than the generic messages, on average. Table 4.6 shows the determinants of dropping out of the SMS program. The personal goal reminder was associated with 12 percentage points more program attrition than the generic reminder, after controlling for baseline covariates. The full results are reported in appendix C. Demographic characteristics (age, gender) are not significantly associated with program attrition. Self-reported motivation problems also do not affect dropout, although a missing value for that question was associated with lower rates of program attrition. The only significant predictor of program attrition is the personal goal reminder treatment, indicating that something about the personal goal reminder itself was disliked by respondents, causing them to unsubscribe from the SMS reminders.

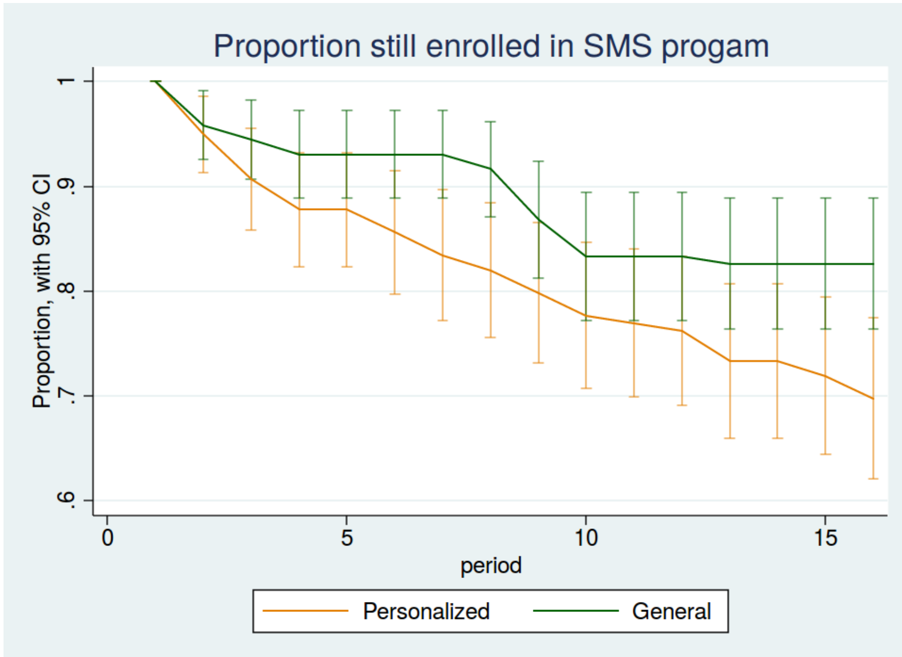

Figure 4.4: Proportion still enrolled in SMS reminders, by SMS group

Table 4.6: Determinants of program attrition

	(1)	(2)
	Dropped out of reminders	
Personal reminder	0.09	0.12**
	(0.056)	(0.056)
Additional controls	N	Y
Gym branch FE	N	Y
Observations	291	290
R-squared	0.013	0.112

Does timing matter?

We conducted a subgroup analysis to evaluate whether reminders sent earlier in the week were more effective than those sent later in the week. We find no significant effects of the reminders, and no differences by reminders early in the week as compared to later. We also estimate the proportion of participants with any gym visit on each day before and after the reminders. We observe a higher proportion of gym attendance on the day of the reminder, but as shown in section 4.3.3 this does not translate into higher overall attendance or higher rates of goal attainment.

Did either SMS reminder have an effect?

Another potential explanation for our results is that both reminders had the same effect (i.e. no difference in effectiveness between the general reminder and personal goal reminder), or that neither had an effect.

Red vertical line indicates the beginning of the intervention period, when the first SMS reminder was sent (May 30, 2016.)

Figure 4.5: Proportion attaining weekly goal over study period

Figure 4.5 shows the probability of attaining one's goal for each week, before the intervention and during the intervention period. We do not see a change in the level or the trajectory when the SMS reminders began. To test this, we use a difference-in-differences (DID) estimator to evaluate the before-after change for both SMS reminder groups.

Table 4.7: Difference-in-differences estimates of SMS reminders

	(1)	(2)
	Outcome: # weekly visits	
Personal reminder	0.07	0.04
	(0.109)	(0.103)
During intervention	-0.29***	-0.31***
	(0.058)	(0.087)
Personal X during intervention	0.03	0.03
	(0.076)	(0.077)
Stratification variables	Y	Y
Additional controls	N	Y
Week FE	N	Y
Gym branch FE	N	Y
Observations	15,618	15,561
R-squared	0.115	0.203

Table 4.7 reports the DID estimates of the effect of the treatment on number of weekly visits. The DID estimator (the interaction of the treatment and the intervention time period) is not significant, indicating no additional effect of the personal reminder in the intervention period relative to before the intervention. The coefficient on “during intervention" represents the difference in the general SMS group before and after the intervention: it is negative, showing that weekly visits fell during the intervention period, which is also reflected in figure 4.5. These results suggest that there was a trend toward fewer gym visits over time, and that neither SMS treatment was effective in increasing gym attendance.

Discussion

Exercise is important for health. Low rates of exercise are associated with a range of health problems and contribute to the chronic disease burden [83, 9]. Even individuals who want to exercise often do not follow through on their intentions to do so. Reminders may be one way to increase rates of exercise, and other beneficial health habits, but more evidence is needed on how to make reminders as effective as possible. In this study, we compared two reminders designed to increase gym attendance. We evaluated the impact of a personal goal reminder, designed to leverage loss aversion, relative to a general reminder about exercising at the gym. We found that the personal goal reminder was not effective in helping people attain their own goals or in exercising more frequently, but that it may have increased gym attendance among members who already performed well relative to their goals.

This study found that reminders were not an effective tool for increasing rates of exercise. This finding contrasts with several studies [96, 97, 98] demonstrating that reminders were effective for exercise. Reminders are an attractive policy tool because they are inexpensive to administer (our per-message cost was only $0.02) and, as we show, are able to be tailored with individuals' personal information.

However, our results suggest that more evidence is needed on how to design the content of reminders to ensure that they are effective. We tested a reminder designed to leverage loss aversion, but reminders focused on goals may work through a more direct pathway if the message containing the goal is simply more motivating than an generic message, or if the personal nature of the message is more salient. We found suggestive evidence that the personal goal reminder may have been ineffective due to calling attention to a goal that was unattainable, which may be why the personal goal reminder had a positive effect among those with high baseline attendance relative to their goals and also caused high rates of program attrition.

Interventions targeted at health behaviors may be able to learn from the evidence about planning and "implementation intentions" [100], which describes the linkage of a set of circumstances to a concrete goal-directed response in order to fulfill an intention. Implementation intentions have been successful in increasing exercise [101, 102]. Similarly, Milkman and colleagues [103] find that interventions targeting planning and scheduling can significantly increase uptake of seasonal flu vaccine. In this study, the goal represents an intention to exercise but our intervention did not focus on planning concretely around how to meet the goal. Therefore, even if the reminders made exercise salient, participants were unable to increase their exercise frequency.

A notable finding from our study is that a very small proportion of participants attain their exercise goals in any given week. On average only 12% of participants meet their goal in a given week and weekly average attendance is 0.77 visits. These figures are in a population of individuals who recently joined gyms and who consented to participate in an exercise encouragement SMS reminder program. It may be that small nudges are insufficient to achieve behavior change in this context. A meta-analysis of interventions to increase physical activity found that interventions were only somewhat effective [10]. Programs that foster social support for exercise were found to be effective in a review by Heath and colleagues [104].

This study has strengths and limitations. The study design, a randomized trial, enables unbiased effect size estimates in a context where the relationship between health behaviors and outcomes is frequently plagued by residual confounding. Specific features of the intervention design required making trade-offs, in particular when considering the frequency of the reminders. It is possible that our reminders were ineffective because they were not sent frequently enough or did not arrive at the right moment to take advantage of the weekly nature of scheduling and goal setting. We were able to use administrative data and to send low-cost SMS reminders, making our approach inexpensive and easily replicable and testable in other contexts. However, using administrative data limits the information we could collect from participants. The limited data on socio-economic and demographic characteristics limits possibilities for sub-group analyses. This intervention focuses on goals measured in weekly attendance, but it is possible that gym attendees develop other types of goals, for example a specific weight loss goal or number of miles they can run, in which case the weekly goal is not as relevant and may render the intervention ineffective. We also had high rates of program attrition. This may have been exacerbated by an IRB requirement to include information on how to unsubscribe in every reminder.

Conclusion

Given the low rates of exercise and the importance of physical activity for both individual and population health, designing effective interventions to increase exercise is an important public health goal. This study tested the effect of a personalized goal-oriented reminder designed to leverage loss aversion. We found that the personal goal reminder did not increase exercise. This study contributes to the growing literature on SMS reminders and underscores the importance of gathering more evidence on the design and content of reminders.

Appendix A: Sample characteristics

Table 4.8: Study participation and treatment assignment by gym branch

Gym Branch	Total Eligible Members	Number of Participants	Personal SMS	General SMS
1	64	32	13	19
2	18	11	7	4
3	69	50	26	24
4	30	16	10	6
5	63	23	10	13
6	33	31	14	17
7	145	60	31	29
8	29	20	11	9
9	66	25	11	14
10	97	47	25	22
11	27	12	7	5
12	42	31	15	16
Total	683	358	180	178

Table 4.9: Characteristics of study non-participants and participants

	(1) Did not consent		(2) Consented		(3)	
	Mean	SD	Mean	SD	Difference	T-stat
Respondent's age	39.21	13.33	37.76	13.46	1.45	(1.37)
Respondent is female	0.35	0.48	0.37	0.48	-0.03	(-0.72)
English as primary language	0.13	0.33	0.09	0.28	0.04*	(1.68)
Respondent is smoker	0.09	0.29	0.12	0.33	-0.03	(-1.29)
Attendance	2.31	0.89	2.31	1.02	0.01	(0.09)
Number of years with goal	3.73	4.41	2.62	3.73	1.11**	(2.54)
Raw goal visits	3.00	0.97	3.00	0.86	0.00	(0.05)
Has motivational problems	0.64	0.05	0.73	0.04	-0.09	(-1.48)
Observations	325		358		683	

**p<0.05, *p<0.1

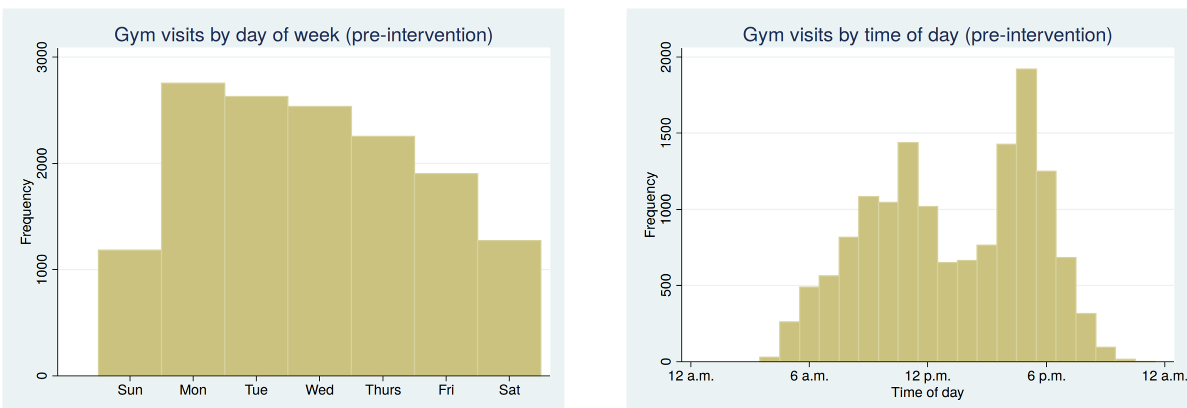

Figure 4.6: Pre-intervention patterns of gym visits by day and time

Appendix B: Full results

Table 4.10: Panel estimates of personal SMS reminder

	(1)	(2)	(3)	(4)
	Outcome:			
	Attained goal		# weekly visits	
Personal reminder	0.01	0.00	0.10	0.06
	(0.022)	(0.021)	(0.110)	(0.101)
Goal: 3x/week	-0.05	-0.03	0.24**	0.26**
	(0.029)	(0.027)	(0.103)	(0.103)
Goal: >3x/week	-0.05	-0.03	0.62***	0.66***
	(0.036)	(0.033)	(0.195)	(0.169)
>3 visits pre-intervention	0.13***	0.11***	0.72***	0.58***
	(0.022)	(0.020)	(0.109)	(0.092)
Age		0.00***		0.02***
		(0.001)		(0.004)
Female		0.03		0.23**
		(0.023)		(0.112)
Has prior gym experience		0.04**		0.30***
		(0.022)		(0.106)
Has motivational problem		-0.09***		-0.42***
		(0.021)		(0.101)
Motivation questions missing		-0.00		-0.11
		(0.037)		(0.186)
Is smoker		-0.00		-0.12
		(0.026)		(0.107)
Smoking question missing		-0.04		-0.29
		(0.060)		(0.306)
Constant	0.08***	0.01	0.10	-0.22
	(0.028)	(0.060)	(0.110)	(0.275)
Week FE	N	Y	N	Y
Gym branch FE	N	Y	N	Y
Observations	9,864	9,828	9,864	9,828
R-squared	0.042	0.106	0.093	0.191

Columns 2 and 4 include week and gym location fixed effects. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 4.11: Aggregate estimates of personal SMS reminder

	(1)	(2)	(3)	(4)
	# Weeks attained goal		Total visits	
Personal reminder	1.44	1.01	4.23	2.98
	(1.229)	(1.142)	(3.819)	(3.666)
Goal: 3x/week	1.89	2.50*	7.67**	9.19**
	(1.482)	(1.428)	(3.562)	(3.718)
Goal: >3x/week	3.19	3.53*	20.01***	21.00***
	(1.987)	(1.881)	(6.678)	(6.144)
>3 visits pre-intervention	8.51***	6.84***	26.49***	21.67***
	(1.237)	(1.221)	(3.864)	(3.423)
Age		0.19***		0.53***
		(0.046)		(0.145)
Female		1.13		5.80
		(1.267)		(3.981)
Has prior gym experience		2.86**		10.01***
		(1.315)		(3.562)
Has motivational problem		-5.25***		-12.99***
		(1.305)		(3.611)
Motivational problem missing		-1.21		-2.35
		(2.129)		(6.436)
Is smoker		-1.09		-2.45
		(1.454)		(3.845)
Smoker missing		-3.72		-8.33
		(3.289)		(10.343)
Constant	5.04***	-0.97	3.71	-15.17*
	(1.441)	(3.166)	(3.680)	(9.096)
Gym branch FE	N	Y	N	Y
Observations	291	290	291	290
R-squared	0.155	0.336	0.175	0.308

Columns 2 and 4 include gym location fixed effects. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

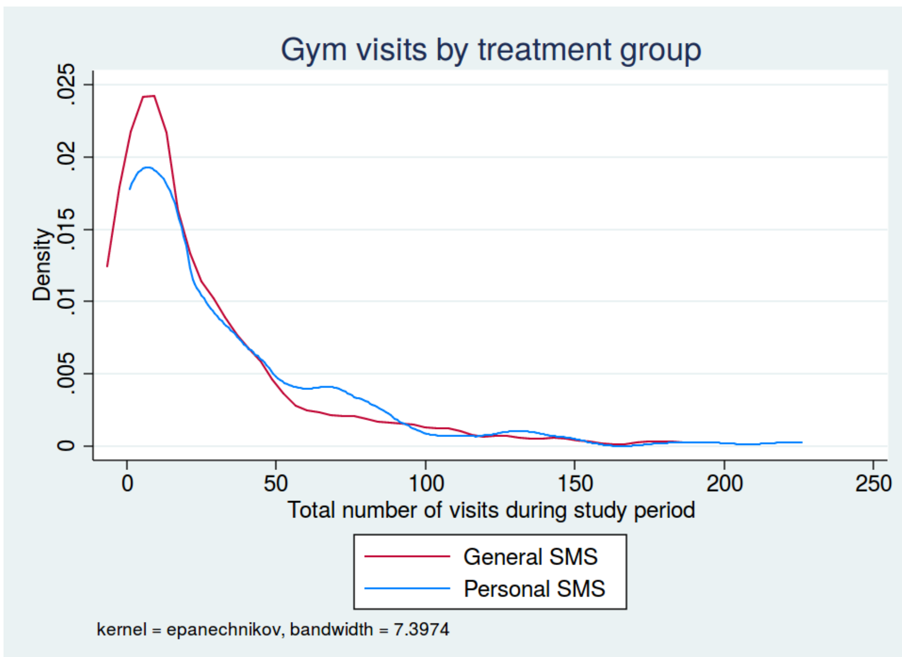

Figure 4.7: Total gym visits, by SMS Group

Appendix C: Mechanism analysis

Table 4.12: Effect of pre-intervention gap between goal and attendance

	(1)	(2)	(3)	(4)
	# Weeks attained goal		Total visits	
Personal reminder	1.62	1.93	8.70	10.67*
	(1.791)	(1.637)	(6.338)	(5.854)
Average weekly gap from goal	3.94***	3.70***	13.47***	13.27***
	(0.725)	(0.684)	(2.240)	(1.948)
Personal reminder X gap	0.44	1.04	4.71	7.47**
	(0.897)	(0.891)	(3.668)	(3.522)
Goal: 3x/week	5.03***	5.46***	19.51***	20.80***
	(1.483)	(1.382)	(3.845)	(3.656)
Goal: >3x/week	9.50***	9.54***	44.04***	45.00***
	(2.026)	(1.819)	(7.381)	(6.210)
> 3 total visits	5.85***	4.24***	16.27***	10.97***
	(1.183)	(1.135)	(3.298)	(2.850)
Age		0.17***		0.45***
		(0.041)		(0.118)
Female		1.95*		9.21***
		(1.150)		(3.426)
Has prior gym experience		2.07*		6.79**
		(1.210)		(3.112)
Has motivational problem		-4.80***		-11.25***
		(1.171)		(2.941)
Motivational problem missing		1.03		7.26
		(1.870)		(5.413)
Smoker		-2.33		-7.41*
		(1.503)		(4.045)
Smoking status missing		-3.31		-7.23
		(2.750)		(8.376)
Constant	8.42***	2.44	14.74***	-4.29
	(1.624)	(3.025)	(4.041)	(7.863)
Gym branch FE	N	Y	N Y	
Observations	291	290	291	290
R-squared	0.309	0.473	0.404	0.539

Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

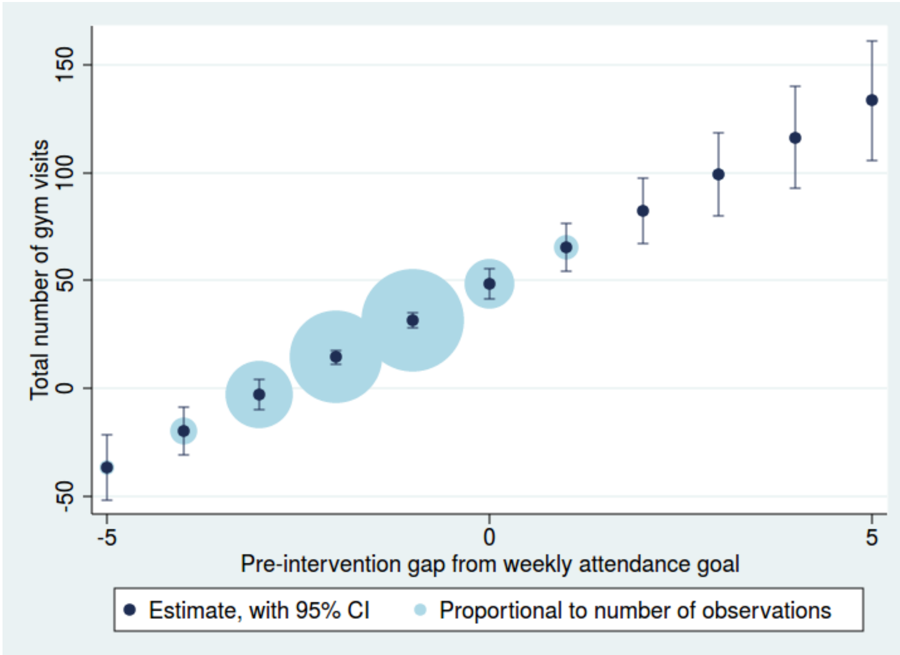

Figure 4.8: Estimates: Average marginal effects by level of pre-intervention gap from goal

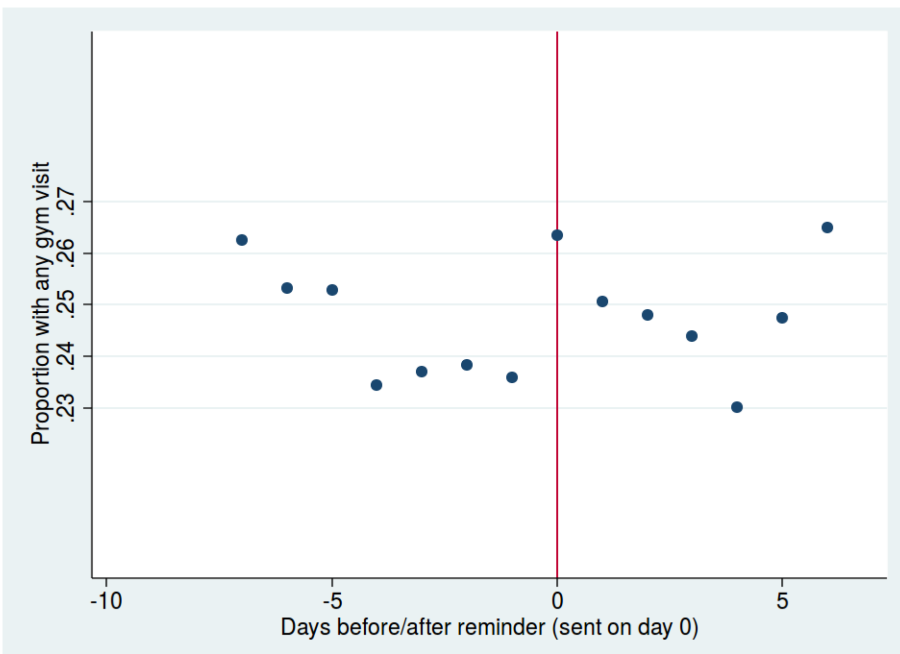

Figure 4.9: Average attendance before and after SMS reminders, pooled across all SMS reminders

Table 4.13: Determinants of program attrition

	(1)	(2)
	Dropped out of reminders	
Personal reminder	0.09	0.12**
	(0.056)	(0.056)
Goal: 3x/week	0.02	-0.01
	(0.067)	(0.071)
Goal: >3x/week	0.04	-0.01
	(0.086)	(0.087)
>3 visits pre-intervention	0.06	0.06
	(0.056)	(0.058)
Age		-0.00
		(0.002)
Female		-0.06
		(0.061)
Has prior gym experience		-0.01
		(0.065)
Has motivational problem		0.02
		(0.067)
Motivation problem missing		-0.38***
		(0.062)
Smoker		-0.04
		(0.082)
Smoking status missing		0.15
		(0.167)
Constant	0.25***	0.52***
	(0.067)	(0.156)
Gym branch FE	N	Y
Observations	291	290
R-squared	0.013	0.112

Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

[1]

Table 4.14: Panel estimates of personal SMS reminder, by day of reminder

	(1)	(2)	(3)	(4)	(5)	(6)
	Reminder on Sun/Mon			Reminder Tues or later		
	Outcome: Number of visits per 15-day period					
Personal reminder	0.13	0.13	0.12	0.30	0.30	0.31
	(0.241)	(0.242)	(0.272)	(0.220)	(0.220)	(0.245)
Goal: 3x/week	0.20	0.20	0.03	0.49**	0.49**	0.35
	(0.300)	(0.300)	(0.337)	(0.243)	(0.244)	(0.262)
Goal: >3x/week	1.39***	1.39***	1.23**	1.20***	1.20***	0.99***
	(0.466)	(0.467)	(0.478)	(0.387)	(0.388)	(0.376)
Goal: n/a	0.65*	0.65*	-0.15	0.75**	0.75**	-0.13
	(0.394)	(0.395)	(0.473)	(0.351)	(0.351)	(0.443)
>3 visits pre-intervention	1.35***	1.35***	1.08***	1.37***	1.37***	1.05***
	(0.235)	(0.235)	(0.273)	(0.205)	(0.205)	(0.231)
Motivation index			-0.25***			-0.27***
			(0.090)			(0.076)
Personal SMS X motiv			0.00			0.03
			(0.144)			(0.130)
Constant	1.00**	1.49***	1.75***	0.63*	0.72*	0.83**
	(0.457)	(0.490)	(0.506)	(0.383)	(0.394)	(0.399)
Period FE	N	Y	Y	N	Y	Y
Gym branch FE	Y	Y	Y	Y	Y	Y
Observations	1,415	1,415	1,085	3,396	3,396	2,604
R-squared	0.120	0.147	0.180	0.117	0.130	0.165

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table 4.15: Difference-in-differences estimates of SMS reminders

	(1)	(2)
	# weekly visits	
Personal reminder	0.07	0.04
	(0.109)	(0.103)
During intervention	-0.29***	-0.31***
	(0.058)	(0.087)
Personal X during intervention	0.03	0.03
	(0.076)	(0.077)
Goal: 3x/week	0.27***	0.29***
	(0.097)	(0.098)
Goal: >3x/week	0.68***	0.72***
	(0.182)	(0.158)
>3 visits pre-intervention	0.81***	0.67***
	(0.102)	(0.087)
Age		0.01***
		(0.004)
Female		0.15
		(0.108)
Has prior gym experience		0.32***
		(0.093)
Has motivational problem		-0.38***
		(0.094)
Motivation questions missing		-0.22
		(0.170)
Is smoker		-0.05
		(0.100)
Smoking status missing		-0.28
		(0.300)
Constant	0.31***	-0.70***
	(0.109)	(0.235)
Week FE	N	Y
Gym branch FE	N	Y
Observations	15,618	15,561
R-squared	0.115	0.203

Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

5

Conclusion

This thesis applied concepts from behavioral economics to three important global health challenges. Paper 1 used a quasi-experimental design and longitudinal survey data to estimate the effect of ART eligibility on household members' HIV status knowledge. Paper 2 used lab-in-the-field data on preferences and behavioral biases to assess the role of present bias and salience in malaria prevention behavior. Paper 3 used a randomized controlled trial of SMS reminders to evaluate the effect of leveraging loss aversion using personal, goal-oriented reminders.

In paper 1, I evaluated the effect of ART on knowledge of HIV status among a patient's family members. The results demonstrate that men are more likely to report knowing their HIV status when they live with a family member who is eligible for ART. This suggests that leveraging family and household relationships may be an effective way to bring more men into HIV testing and care. This would further global efforts to reach the UNAIDS 90-90-90 targets and end the HIV epidemic. Following on these findings, my future research will evaluate the effects on later cascade-of-care outcomes.

In paper 2, I studied the association between time preferences, salient illness events, and malaria prevention. I found that individuals who experienced a salient illness were more likely to spend on malaria prevention, consistent with a model of where inattention to health would explain low rates of preventive behavior. These findings suggest that interventions targeting routine, habitual preventive behaviors may be more effective if they are designed in a way that makes health salient, or that capitalizes on salient health events to draw attention to related health behaviors.

In paper 3, I tested the effect of a personalized, goal-oriented reminder on gym attendance and goal attainment. The personal goal reminder did not increase exercise, and may have discouraged some participants. This study underscores the importance of the content of SMS reminders and highlights the necessity of correctly designing and targeting behavioral interventions.

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